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THE
PATHOLOGY OF FEVER;
BEING THE SUBJECT OF THE
GULSTONIAN LECTURE,
LATELY DELIVERED AT THE
ROYAL COLLEGE OF PHYSICIANS.

BY J. R. PARK, M. D.

FELLOW OF THE ROYAL COLLEGE OF PHYSICIANS.

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PATHOLOGY OF FEVER

GUSTAVIAN LECTURE

ROYAL COLLEGE OF PHYSICIANS

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PREFACE.

AN opinion appears to prevail, that the only proper mode of physiological research is by experiment on living animals ; that reasoning by induction from the ordinary phenomena of life is inadequate to that purpose ; and that tracing out analogies is unphilosophical.

As the views offered in the following treatise are founded on principles deduced from facts which rest upon observation alone, and are not obtained by experiment, some notice of this opinion appears necessary.

Without disparagement to the utility of experimental inquiry in general, it may with confidence be asserted, that it is not equally applicable to all the purposes of scientific research ; and the object of the following treatise is one of those to which it is least adapted, if not wholly inapplicable.

The process required for ascertaining the cause or agent by which certain effects are produced, is widely different from that which is requisite for tracing out the order in which the phenomena succeed each other, and noting the circumstances which tend to modify or influence their mode of succession. Nor are these necessarily connected as subjects of investigation; the knowledge of the cause being not at all required for discovering the principles which regulate its effects. It is no more necessary that we should ascertain the cause of sensation and motion, the primary attributes of living matter, in order to deduce the laws which govern their phenomena, than it is requisite to ascertain the cause of attraction and gravitation, in order to trace out the laws of inanimate matter. The laws of life and those of matter may both be developed, though the cause of each remain equally unknown.

Admitting the agent or cause of vital phenomena to be a legitimate object of research, and allowing experiment to be the means best adapted for its attainment, it does not follow that the same means are equally suitable for

deducing the laws by which these phenomena are governed. On the contrary, experiment appears to be wholly inapplicable to this purpose, which can be effected only by careful observance of the phenomena, and by inferences fairly and legitimately drawn from them.

Nor will any one, who professes to be a follower of Bacon or of Newton, hesitate to decide which of these two objects of research should properly take precedence in the order of inquiry. Whether it be more scientific to commence with inquiring into the cause, and thence proceed to explain its effects; or by attentively examining the effects first to deduce the laws, and then reason from the effects up to their cause.

The latter is the plan which the author, esteeming it the more philosophical, has invariably pursued; and conceiving the laws of sensation and motion to be subjects of research neither minor in importance to that of ascertaining their cause, nor secondary in the proper order of inquiry, he has formerly devoted to that purpose a series of essays published in the *Quarterly Journal of Science*,

Literature, and the Arts. The object of the present treatise is the further development of the same principles in the application of them to illustrate the phenomena of fever.

If experiment was deemed inapplicable to the purpose of deducing the principles on which the subsequent doctrine is founded; still less is it calculated to afford assistance in applying these principles to the investigation in question; an inquiry in which observation and reasoning can alone avail.

Of facts, and explanations of facts, all our knowledge may be said to consist. Science is chiefly composed of the latter; facts are only the materials employed in reasoning. To object to reasoning is therefore to object to science itself.

The proper mode of reasoning is indeed a question of material importance. The hypothetical mode is with justice exploded, being founded on assumption instead of fact. The synthetical plan, which begins from known causes, and thence proceeds to explain their effects, is likewise considered objectionable, as inverting the proper order of inquiry; and as founded moreover on a partial

statement of facts. The analytical is allowed to be the only legitimate mode of research; in which the inference is drawn from a general survey of all the phenomena, and shown to accord with them all.

Such was the method employed in establishing the principles on which the following views are founded; nor is that plan deviated from in applying these principles to illustrate the nature and varieties of fever.

To object to the use of analogy in scientific inquiry is no less absurd than to object to reasoning.

After establishing general principles, it is indispensably necessary to show the modifications which they present; or, in other words, to trace out the analogy between parallel phenomena, which are subject to similar laws, but modified by particular circumstances.

Thus, when the phenomena of voluntary motion have been traced up to certain principles, which regulate the order of their succession, those of involuntary motion are compared with them, and shown to be analogous,

or governed by the same laws slightly modified.

In fact, analogy may be employed either as a mode of illustration or of argument; and if the latter use be objectionable, the former is often indispensable, for in many cases the science of physiology neither possesses any better, nor admits of any other. Pathological science, too, necessarily abounds in analogies, which are only objectionable when vaguely and loosely drawn.

Thus the phenomena of inflammation vary in every different texture, and those of fever are equally diversified; yet these varieties in each are considered analogous, from a supposed similarity in the laws by which they are governed.

But the laws themselves in these instances have never been clearly ascertained, and consequently the supposed analogies cannot but be vague and uncertain, and little calculated for the purposes either of illustration or argument.

Although experiment can do no more than furnish facts to reason from, and is therefore

inapplicable to the purpose of tracing out laws or principles, yet it may with justice be urged, that one of its legitimate objects is to bring to the test the truth of principles previously deduced.

Accordingly, to this test will the truth of some of those employed in the following investigation be submitted in the Supplement. Not indeed to the test of experiments made by the author himself, whose fidelity might, in that case, be called in question, but to such as leave no ground for a suspicion of this kind.

And should it appear that these principles not only accord with the results of the experiments in question, but are moreover capable of reconciling apparent contradictions which the experiments involve, this will surely be considered as no slight confirmation of their truth.

BEDFORD SQUARE, 1821.

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PATHOLOGY OF FEVER.

GENERAL PRINCIPLES.

THE ultimate object of pathological research is to improve the practice of physic, by throwing light on the nature of disease, and enlarging the bounds of medical science. And no subject is better calculated to effect that purpose than an inquiry into the phenomena of fever. For being an affection in which every organ participates, and every function is deranged, its explanation involves nearly all the principles of pathology, and presents an epitome of the science.

In fact, the most important pathological principles might be deduced from the phenomena of fever alone. But since inferring these principles from febrile phenomena,

and afterwards explaining the nature of fever upon the same principles, would be reasoning in a circle, the author has sought to avoid this imputation, by establishing the principles of his pathology on a distinct and independent basis.

His first attempt was submitted to the public about ten years ago, under the title of *An Inquiry into the Laws of Animal Life*; but a revised and enlarged statement of his views has been recently published in the earlier numbers of the *Quarterly Journal of Science, Literature, and the Arts*, edited at the Royal Institution.

As the author, however, is not entitled to presume that his readers are already acquainted with his former publications, a brief abstract of the doctrines immediately connected with the present inquiry will be given in the following pages; but, at the same time, he entreats that their truth and accuracy may not be appreciated by this partial statement alone.

The present imperfect state of medical science is so generally admitted, that he scarcely expects to be called upon to vindi-

cate the propriety of questioning the truth of principles which are generally received. It must surely be esteemed presumptive proof of some latent error, when the received principles of a science are found inadequate to solve the most interesting of its problems; and in no branch of pathology is that inadequacy more conspicuous than in the doctrine of fever.

But that he may not be charged with assuming a point that he should rather have proved, a few of those problems may be enumerated, which the writers most eminent for their labours in this department of the science have candidly acknowledged their inability to solve.

In the first place, then, it has never been satisfactorily shown why fever is at one time continued, and at another intermittent. Further, it remains to be explained why each of these forms is again divided into different species, marked by certain characters which are sufficiently distinct and uniform; thus, one form of continued fever is preceded by a severe cold stage, while in another the cold stage is slight, or altogether wanting. So

in one form of intermittent the paroxysm returns daily, in another on alternate days, while the third passes over two days before its recurrence. Moreover, in each species the paroxysm is disposed to renew its attack at a particular hour of the day; the quotidian usually occurring in the morning, the tertian about noon, while the quartan is more commonly postponed till evening.

The types of fever are ascribed by Cullen, whose doctrine is the most prevalent, and perhaps the least exceptionable of any, to the tendency to a diurnal revolution observable in the animal economy; which he assumes as an ultimate fact, or law of nature. But after assuming this principle, he nowhere attempts to trace out its operation, or account for the varieties it presents;—thus tacitly admitting his inability to solve this problem.

The stages of fever are supposed by Cullen to depend upon each other; the cold producing the hot, and the hot the sweating stage.

Upon this principle some relation should be found to obtain between them; or when the cold stage is long and violent, the hot

should bear some proportion to it. But it happens that the exceptions to this rule are more numerous than its examples; the longest cold fit, as in quartan ague, being followed by the shortest hot fit; while the severest hot stage, as in typhus, is preceded by the slightest cold stage.

But presuming that these are sufficient to show the inadequacy of the received doctrines of fever to afford a satisfactory solution of its phenomena, we may now proceed to inquire whether the principles deduced in the Essays already alluded to, are capable of affording that solution.

Of these the first and most important is that which regards—*the connexion of every function with, and its immediate dependance upon, the actual state of circulation in the organ that performs it.*

The important connexion between circulation and sensibility was formerly noticed by Hunter and by Bichât. But the former assigned the final instead of the physical cause, by ascribing the change of circulation to a necessity for it; and the latter regarded altered circulation as the effect and

not the cause of altered sensibility ; being just the converse of the principle here contended for, which is, that the state of sensibility immediately depends upon the state of circulation.

Without disputing the agency of nerves as the organs of sensation, the actual state of the nerve appears to be subject, like that of every other organ, to the state of circulation in its minute or capillary vessels, which is liable to variation both at its origin in the sensorium, and at its sentient extremity. In short, nerves enter into the texture and form a part of every organ, and there appears no use in their being separately considered in regard to the operation of a law which is alike common to all organs ; for whatever be the nature and function of the part, it will be found that a change of its vital powers is the effect that results from altered circulation.

If the organ be a sentient organ, change of sensibility ensues ;—if an organ of motion, change of mobility ;—if a secreting organ, change of secretion ;—if the organ of mind, change of the mental faculties : diminution

of its powers being the consequence that results if the circulation be reduced ; and augmentation of them, to a certain extent, if the circulation be increased ; as the following instances may illustrate.

In sentient organs the faculty of feeling is impaired, and the sensibility of the surface is benumbed, when its vessels are constricted by cold.

In moving organs, the power of motion is impeded, the fingers become torpid, and the activity of the limbs is suspended, when their circulation is obstructed by the same cause.

The mental faculties show the same change when circulation is impeded or diminished in the brain : hence the listlessness or state of apathy that attends the diminished circulation accompanying sea-sickness ; hence the torpor and loss of energy produced by excessive cold ; and hence the total failure of the mental powers that proceeds from loss of blood, and is the precursor of syncope.

Secretion, if not entirely a vascular process, is manifestly dependent upon circulation, and fluctuates with every change in

the state of the secreting vessels. Hence the thin watery secretion from the nose, when the vessels of its mucous membrane are constricted by cold.

Such are the effects of diminished circulation, or over-contraction of the capillary vessels. On the other hand, increase of the vital powers will be found to result from the reverse of this state, or from increased circulation in these vessels, as in the following instances.

The sense of feeling is rendered more acute on the surface, when circulation is augmented by warmth; and becomes excessive and painful in any part when its circulation is inordinately increased by inflammation.

The powers of motion become more active when circulation is augmented in the muscular system, as we grow warm with exertion; and the mobility of the muscles becomes excessive and painful when their circulation is inordinately increased under inflammation. Hence the spasmodic pains attendant upon rheumatism, pleurodyne, and inflammation of the intestines.

The mental faculties obey the same law;

the activity of the mind is augmented by moderate increase of circulation in the brain; but confusion of ideas arises when the blood is too freely determined to the head by intoxicating liquors, or in the delirium of fever.

Secretion and exhalation are also augmented by moderate increase of circulation in their respective organs; hence the abundant secretion of saliva occasioned by taking food, and the flow of perspiration when blood is moderately determined to the surface by active bodily exertion.—Why these functions are suppressed by inordinate increase of circulation, will be explained hereafter.

These facts may serve to illustrate the influence of altered circulation, upon which the phenomena of fever chiefly depend.

But to form a distinct idea of the manner in which the various changes of function, and the morbid sensations that accompany the febrile state, are produced, it is necessary to observe further a corollary that follows from this principle.

Not only has every organ a different function to perform, but every separate texture has moreover a mode of feeling exclusively

belonging to itself. Thus the sense of itching is peculiar to parts covered with cuticle; the sense of heat or cold is chiefly confined to the surface; while that of thirst and nausea belong to the stomach; the sense of lassitude occurs only in the muscles, while that of aching is peculiar to bone.

Now all these different feelings are liable to be called forth, without any external cause to awaken them, when change of circulation takes place in their respective organs. Thus itching attends psora, urticaria, and various other cutaneous eruptions; the sense of cold or heat accompanies the febrile state; that of lassitude is attendant upon rheumatic affections; and aching arises from nodes and other affections of the bone or periosteum.

But every part having a different mode of feeling, and a different function to perform, it follows as a corollary, that various effects will result in different organs from the same change of circulation. And accordingly, that state of vessels which occasions a sense of *cold* on the surface is accompanied by the sense of *nausea* in the stomach; while that which causes the sense of *heat* externally,

produces that of *thirst* internally ; the same morbid increase of circulation which occasions a painful sense of *lassitude* in the muscles, produces *confusion of ideas* in the brain : and

On this principle are the morbid sensations and changes of function explicable, which are attendant upon the febrile state ; and thus we advance one step towards accounting for its phenomena.

But the most important part of the problem still remains to be solved ; and this is to ascertain the cause of the changes of circulation that give rise to these effects.

This question is intimately connected with a circumstance noticed by different authors, but not accounted for by any that the writer is acquainted with, namely, the tendency to a diurnal revolution or periodical change of action, observable in the animal economy.

The fact has been noticed by different physiologists. Cullen, as before stated, assigns it as the cause of the febrile types ; and Bichât has noticed it under the term periodicity of action.

Neither of these authors, however, inquire

into its origin, but content themselves with assuming it as an ultimate fact; and they moreover restrict its influence to the organs of animal life, or the functions of voluntary motion and mind.

Now the phenomena of fever manifestly have their immediate seat in the vascular system, which belongs to the class of involuntary functions; little aid then could be derived from this principle in accounting for them, were its operation, as these authors maintain, confined to the brain and voluntary organs.

But in this opinion there seems no reason to concur. On the contrary, the laws of motion appear to be nearly similar in all moving organs; *all having certain limits affixed to their powers, and all, in consequence of this limitation, being subject to periodical change of action.*

The two classes differ indeed in several respects, and this difference it is essential to notice, as it defines the limits of the principle proposed, and contributes moreover to illustrate the nature of vascular action.

The points of difference relate to their

power and mode of action ; the period of its duration ; the degree of repose enjoyed by each class ; and the circumstances attending this state of repose.

In the organs of voluntary motion the degree of exertion, and its duration, depend upon the will, and the sense of fatigue is sufficient to prevent their efforts from being protracted to an injurious length.

But in organs not subject to the control of the will, this sensation would be superfluous ; and their exertion, which is at all times nearly uniform, is prevented from being continued to an injurious degree by the spontaneous relaxation of the organs themselves.

The circumstances that denote this spontaneous relaxation are various in different parts of the body. In the vascular system it is seen by the eyes growing red, and the face becoming flushed as the hour of sleep approaches, while the feet are apt to swell towards evening, from the same cause. The relaxation of the heart appears in the retardation of the pulse at stated periods ; for, as noticed by Haller, Hunter, Whytt, and

others, the circulation becomes slower during sleep. Relaxation takes place in the stomach, and continues for some time after taking food; and if this should not be considered as spontaneous, the sense of hunger, which appears to proceed from painful contraction in that organ, is often known to cease after a certain time, although no food has been taken, from the spontaneous cessation of the action that occasioned it.

Thus involuntary as well as voluntary organs manifest a periodical change, and spontaneous remission of action at stated periods.

The repose they enjoy is not indeed a total suspension of action; but it is sufficient to account for the renewal of activity which they derive from it, as appears from the following considerations.

The heart, when we lie down, is relieved from a burthen estimated by Dr. Hales at fifty pounds, the difference between the force required to keep up the circulation in the erect and the horizontal posture. While circulation is retarded during sleep, breathing becomes slower in consequence, and thus the organs of respiration enjoy a partial re-

pose. Digestion cannot proceed with the same activity by night as by day, since we often fast twelve or sixteen hours together, from evening to morning, while the third part of that time is usually sufficient to bring back the sense of hunger during the day.

Thus the organs of circulation, respiration, and digestion, as well as those of voluntary motion, obtain a degree of rest during sleep. And there is no reason to doubt that this temporary remission of their efforts is as necessary to the one as to the other class of organs, to restore their activity, and fit them for the healthy performance of their function.

There appears then no reason for supposing the operation of this law confined to the organs of voluntary motion, but the alternation of action and rest seems to be common to involuntary as well as voluntary functions. And this limitation of power extending to the vascular system, the periodical changes of circulation which result from it will afford material assistance in explaining some of the leading phenomena of fever.

But another circumstance yet remains to

be noticed before we can apply this principle to the illustration of fever; and it is one which more clearly shows the analogy between the voluntary and involuntary organs. It is, that,—

In the one as well as in the other, whatever increases their ordinary exertion, or augments their previous contraction, increases also in an equal degree their subsequent relaxation, when the cause that excited them to over-action is withdrawn, as the following examples may show.

The sudden but transient constriction of the vessels of the face, occasioned by fear, is quickly followed by an increased flush when the cause is withdrawn, denoting the subsequent relaxation of these vessels.

The thin watery secretion from the nose produced by exposure to cold, denoting increased contraction of the secreting vessels in the mucous membrane, is succeeded by a sense of fulness and heat that bespeaks the opposite state, or that of subsequent relaxation and distension in the same vessels.

The total suppression of urine which sometimes occurs in hysteria, and is proved by

the fruitless introduction of the catheter, to proceed from the kidneys, and not from the bladder—denoting therefore a morbid constriction of the secreting vessels—is followed by a secretion remarkably copious, sometimes amounting, in a few hours, to many quarts—denoting a proportionate relaxation of the same vessels.

And in like manner appear to be connected the inordinate contraction of the capillary system, that will be shown to prevail in the cold fit of fever, and the subsequent relaxation of the same vessels in the hot stage.

Such then is the basis of the doctrine proposed.

The cold stage is regarded simply as a state of undue contraction in the capillary system, (leaving at present out of the question the manner in which this state of vascular constriction is produced,) and the derangements incidental to this stage are considered as the effects of the state of capillary contraction.

On the other hand, the hot stage which follows, is regarded as a state of subsequent relaxation and distension of the same vessels ;

and the various changes of function that characterise this stage, as the consequences of this state of capillary distension.

Proofs that these different states of circulation do actually prevail in the two stages of fever, will be offered presently. But we must defer inquiring into the primary cause of each till we come to consider their several varieties and modifications ; for there is no warrant for concluding that the primary cause is the same in all species of fever. On the contrary, it will be found to vary in each, and hence the varieties in question chiefly proceed, as will hereafter appear.

But we have yet to state what is regarded as the nature of the third or sweating stage, which is considered as forming part of every well marked febrile paroxysm.

The sweating stage is indeed more properly the solution of the paroxysm ; and the reason of that solution, or why transpiration returns, will be rendered evident, by ascertaining the cause from which it was obstructed.

Now this obstruction will be found to proceed in all cases from one and the same *immediate* cause ; namely, from a spasmodic

constriction of the pores or mouths of the exhalent vessels.

In whatever way the morbid distension of these vessels that accompanies the hot stage may be occasioned, a spastic stricture of their mouths appears to be invariably an attendant upon that morbid distension.

Dr. Cullen refers the obstructed transpiration in the hot stage to a partial continuance of the same spastic stricture of the extreme vessels that prevailed in the cold fit; but this stricture is here considered as confined to the mouths of these vessels, a class of organs which were not called into action before, and which now become constricted in consequence of the vessels themselves being relaxed and over-distended.

Contraction of the exhalent vessels, instead of producing dryness on the surface, often has, when moderate in degree, an opposite effect, or occasions a moisture on the skin, for reasons that will presently appear. It is only in extreme cases, or when that contraction is so great as to prevent the fluids from being transmitted to their mouths, as in

the cold fit of ague, that transpiration is suppressed by it.

During the hot stage of fever, that these mouths are closed can hardly admit of dispute, as it seems impossible in any other way to explain, when the vessels to which they belong are gorged and distended with fluids, what prevents these fluids from transuding at every pore.

In order to explain why they are thus constricted in the hot stage, and to understand the nature of these patulous mouths, which perform an important part in the production of both fever and inflammation, we have only to regard them in the light of sphincters, to which they are perfectly analogous.

The office of both is to retain the contents of the organ to which they belong, or to allow their transmission only at suitable times, and in proper quantity.

Accordingly, *the action of the sphincter appears to be vicarious with that of the organ, as its office requires it should be; that is, the sphincter relaxes when the organ contracts, and, on the other hand, the sphincter contracts when*

the organ relaxes. Thus, for example, when the stomach is roused to inordinate efforts of contraction by an emetic, its sphincter, the pylorus, relaxes, and bile is transmitted through the duodenum in vomiting. When the intestines are excited to contraction by a strong purgative, the sphincter of the rectum relaxes, and fæces are with difficulty retained. When the bladder contracts for the expulsion of urine, its sphincter relaxes, and allows the transmission of this fluid. So likewise when the vessels of the surface shrink, and paleness is produced by fear, a cold sweat breaks out, and the surface becomes moist. Or when the vessels collapse from loss of blood, the same clamminess of the surface attends, and is the forerunner of syncope; and thus transpiration is increased, and not suspended, as Dr. Cullen supposed, by moderate contraction of the vessels.

On the other hand, the sphincters and the pores alike become constricted when the organs or vessels to which they belong are relaxed and over-distended. Thus, when the stomach is distended with food, the pylorus closes, and suffers nothing to be transmitted

till the bulk of its contents is reduced by absorption. When the rectum is over-distended by immoderate accumulation of fæces, its sphincter contracts, and the most obstinate constipation is apt to ensue. When the bladder is over-distended with urine, its sphincter becomes constricted, and strangury is the consequence. In like manner, when the vessels of the surface are over-distended by immoderate determination of blood in fever or inflammation, the pores then become constricted, and transpiration is suppressed. And for the same reason, the secretion of bile is suppressed in active inflammation of the liver; and that of urine in acute inflammation of the kidneys.

Thus the mouths of the exhalent vessels terminating on the external and internal surface, appear to be governed by the same laws, and to exhibit the same modes of action as the sphincters belonging to larger organs.

In the hot stage of fever, then, transpiration is suppressed, and morbid heat kept up by over-distension of the vessels exciting spasmodic constriction of their mouths; and accordingly it is to the removal of vascular dis-

tension that we must look for a cessation of that constriction, and the return of transpiration.

Dr. Cullen ascribes the return of transpiration to the force of the reaction overcoming the spasm of the extreme vessels ; but Dr. Currie, in the Medical Reports, objects to this doctrine, and states, that he has often seen the morbid heat obstinately kept up by the violence of the reaction ; and, on the contrary, quickly subside, and transpiration return, when the force of the reaction was reduced by cool air, by blood-letting, or by the affusion of cold water.

It may certainly be questioned whether any instance occurs in which spasm is overcome by force ; and there is reason to concur with the opinion of the author of the Medical Reports, that reducing the reaction, and thus lessening the distension of vessels, is a much more likely mode of effecting this purpose.

But as the morbid distension is occasioned by different causes in different forms of fever, so various means are adapted in each for its removal. In one case, copious depletion, by bleeding or otherwise, may be proper.

In another, the affusion of cold water, by its constrictive influence on the vessels, may best answer that purpose ; the remedies varying as the cause of the fever varies.

In one form of fever this spasm subsides spontaneously, and transpiration returns, after a few hours. In another, the morbid heat continues for many days or even weeks together, with scarcely any remission in its severity.

The cause of these varieties will be shown hereafter ; but we are first called upon to exhibit the proofs that these different states of circulation actually prevail in the two stages of fever ; and moreover to detail the facts from which we infer that the change in each is not, as some suppose, partial, or confined to the surface, but extends to all parts ; being, as Dr. Currie has shown in his Medical Reports, universal.

The proofs of this universality, in parts which are not open to inspection, as internal organs, must be sought for in the changes of function attending ; but collateral proofs are not wanting, and will be adduced in evidence as we proceed.

The difference between the stages, and the universality of the change of circulation in each, will be best shown by contrasting their symptoms.

In the cold stage, the sensation of cold, so distressingly felt on the external surface, is indeed scarcely perceptible on the internal, because this mode of sensation does not belong to the stomach, which is little sensible to altered temperature; but the state of its vessels appears from a circumstance noticed by Dr. Currie, namely, that warm liquids are most grateful to the patient at this time, whereas, on the contrary, cold liquids are preferred in the hot stage which follows.

The mode of sensation peculiar to the stomach, and which denotes a collapsed or constricted state of its vessels, is that of nausea, which usually occurs at the commencement of a febrile attack.

That the sense of nausea is connected with a constricted or collapsed state of vessels, appears from the fact, that this sensation accompanies such a change of circulation, by whatever cause produced. Thus, sickness may proceed from mechanical injury of the brain,

from an active purgative, from loss of blood, or any cause that suddenly reduces circulation; and on the same principle it accompanies the cold stage of fever.

The condition of the vessels in other internal organs may be learned from the state of the kidneys, as shown in the appearance of the urine; which is pale and limpid in the cold fit, but becomes high coloured in the hot, and deposits a copious sediment in the sweating stage.

The different states of the brain in the two stages, is shown in the condition of the mental faculties. In the cold stage they are torpid and oppressed, denoting impeded circulation. In the hot stage they are hurried and confused, indicating morbidly increased circulation.

Delirium occasionally attends both, but with this difference, that in the cold stage the intellectual faculties are impeded from inactivity; while in the hot they are confused from excessive activity, or from an uncontrollable rapidity in the association of ideas.

But the different states of circulation may be learned from the appearance of the eyes,

which derive blood directly from the brain, and clearly indicate every change it undergoes. In the cold stage a leaden dulness is the appearance they put on, whereas in the hot they become red and fiery.

In the muscular system, obstructed circulation appears in the cold stage, from the numbness, torpor, and paralytic tremors, similar to the effects produced by fear, and by external cold. On the other hand, a painful and unceasing restlessness prevails in the hot stage; denoting the reflux of blood to the capillary system, and its inordinate accumulation in these vessels; such being the effects shown to arise from these opposite states of circulation.

But while other moving organs experience this change of mobility from altered circulation, an important question arises whether the heart also participate in the same.

That the heart is subject to the same general law, or liable to have its mobility augmented by increased circulation in its capillary system, is shown by the quick pulse attending certain organic affections. And the permanent acceleration of pulse which

accompanies some forms of fever, continuing for days or even weeks, with little abatement, can hardly be accounted for otherwise than upon this principle, or upon an altered irritability of the organ itself.

The cause which Dr. Cullen assigns for the quick pulse in the hot stage, though satisfactory enough in some forms of fever, does not appear applicable to others.

He ascribes the increased action of the heart to the quantity of blood thrown inwards, and accumulating in the larger vessels during its expulsion from the smaller. A cause adequate, no doubt, to excite this reaction, as it appears to do in other instances.

But if this alone be the cause of its increased action in fever, why does not that action subside again, as in other instances, when the cause is removed. Or when the blood flows back to the smaller vessels, and accumulates there in greater abundance than before ; when, according to Dr. Cullen's own reasoning, the larger vessels must be left comparatively destitute ; why does not the reaction subside again, but continue for weeks together, as it does in some forms of fever ?

A change of mobility in this organ seems the only satisfactory way of accounting for such an effect ; and the state of the pulse in the cold as well as the hot stage, well accords with the respective states of irritability which these changes should produce.

In the cold stage the pulse is slow, labouring, and oppressed, denoting impaired mobility in the heart. Whereas in the hot stage it is greatly accelerated, denoting increased mobility. Changes which agree with those that occur in other moving organs under similar states of circulation.

Why this acceleration of pulse is greater, and continues more permanently in one species of fever than in another, will be considered when we treat of its varieties, which will be the next subject of investigation.

VARIETIES OF FEVER.

SYMPATHETIC OR SYMPTOMATIC.

THE phenomena of fever which have been hitherto considered, are such only as are common to all its varieties; the cold, the hot, and the sweating stage, belong, in some degree, to all fevers, and are considered essential to the formation of a well-marked febrile paroxysm.

But these are materially diversified in each particular form of the disease, and are moreover accompanied by other circumstances, which are highly important in a practical as well as pathological point of view, since they diversify the treatment, while they distinguish the character of each.

To these we shall now direct our attention, and endeavour to develope their nature and cause.

In pursuing this inquiry, we are not called upon to adhere to any established order, or nosological arrangement.

The object of nosology is to classify diseases according to external and visible characters, while their nature remains unknown, and their cause undiscovered. The object of pathological inquiry is to investigate their nature, and ascertain their cause; and it cannot be supposed that the same arrangement will be equally applicable to both, when the end proposed is so different.

We shall therefore adopt that order which seems best suited to the purpose, beginning with the form of fever most calculated to throw light on those which follow.

Fevers have been distinguished into symptomatic and idiopathic, or those which arise from local inflammation, and those in which the fever is supposed to be the primary affection. And this arrangement seems as well adapted to our present purpose as any other.

Beginning with symptomatic fever, the first in the order of its symptoms is the cold stage, the nature of which we have to ascertain. As far as the writer is aware, no satisfactory

reason has yet been assigned why fever from local inflammation is preceded by a rigor or cold fit.

We certainly cannot in this instance adopt the explanation of the cold stage given by Dr. Cullen, namely, “ *a collapse of the extreme vessels, owing to an atony or debility produced by certain sedative powers applied to the brain.*”

Whatever reason there may be for assigning debility as the primary cause of fever in some instances, there appears no ground for such a conclusion in fever from local inflammation ; for example, when it arises from mechanical injury, or surgical operation, in a person previously in full health and vigour.

There is no indication of weakness or want of tone in the symptoms, nor any reason for supposing that sedative powers have in this instance been applied to the brain to produce it. On the contrary, the severity of the fever is in the direct ratio of the tone that prevails, and a primary object in the cure is the reduction of that tone.

Symptomatic fever, as its name implies, derives its origin from the general sympathy of

the vascular system in a local change of action. It is therefore to the laws of sympathy that we must look for its explanation ; and in these we shall find the reason why it is preceded by a rigor or cold fit.

The nature of vascular sympathy, or the general consent of action that prevails throughout the capillary system, has been fully elucidated in the Essays before alluded to ; but its existence being admitted as a fact, is sufficient for our present purpose. And in order to exemplify its mode of operation, we need only call to mind the familiar instance of the effect produced by immersing the feet in hot or cold water, which soon occasions a general glow and distention, or a general cold and shrinking of the capillary vessels.

Upon the same principle may be explained the formation of general fever from local inflammation ; and, in the first place, we have to inquire how this sympathy operates in producing a cold fit.

Its mode of operation appears to be best illustrated by observing the changes that take place, when inflammation occurs in parts that are within the reach of examination.

In Dr. Scudamore's Treatise on Gout, a case is mentioned of active inflammation in the back of the hand, in which the temperature of the inflamed part was found by thermometrical experiment to be several degrees above the standard of health, while that of the fingers and parts contiguous was as many degrees below this standard; the circulation being diminished in the latter as much as it was increased in the former; which admits of the following explanation.

The vessels of the inflamed part being gorged and over-distended with blood, are roused to unusual efforts of resistance to relieve themselves from the load that oppresses them.

But their efforts are unavailing, and distention is not removed by them, or the inflammation would subside; on the contrary, the struggle is ineffectual, and only tends to keep up active circulation, in fact, constituting inflammatory action.

That this conflict or struggle of the overloaded vessels attends the formation of inflammatory action, and may be regarded as one of its primary causes, is an idea suggested

in the writings of John Hunter; who observes, in speaking of the gout, that the pain of the inflamed part does not arise from the mere distention of its vessels, but from their efforts to resist that distention. Accordingly the pain subsides when the struggle ceases, although the distention or swelling often increases at the same time.

Now these efforts are unavailing in the inflamed and weakened vessels, and their distention consequently continues, swelling being a leading symptom of inflammation. But according to the laws of vascular sympathy, or the general consent of action that prevails, other parts participate in the efforts made by the inflamed vessels; and while these efforts are ineffectual in overcoming distention in the weakened vessels of the part inflamed, they are not so in the healthy vessels of the parts contiguous, whose tone is unimpaired, and which therefore effect that contraction which the others were unable to accomplish. The blood is consequently expelled from the healthy vessels, and their circulation is diminished, while the inflamed vessels remain distended as before; and thus

the variation of temperature is produced, noticed by Dr. Scudamore.

To apply this to the doctrine of fever, we have to observe, that the effects described are only confined to the vicinity of the part inflamed, when the local change of action is inconsiderable ; but when sufficiently powerful, they reach to all parts ; a general participation is then excited, according to the laws of vascular sympathy, and thus a general contraction of the healthy vessels ensues, constituting a cold fit.

The rigor of inflammatory fever, then, instead of arising from atony, or loss of tone, according to Dr. Cullen's idea, seems rather to derive its origin from the excess of tone in the constricted vessels, or at least from the unequal distribution of power between the vessels of the healthy and those of the diseased parts ; in consequence of which the simultaneous effort of both is productive of a different result in each ; in one producing inordinate contraction, in the other keeping up inflammatory action.

Accounting in this way for the production of the cold fit, the principles already stated

are sufficient to explain the hot stage that follows. As the former consists in a morbid contraction of the capillary system, and the latter in an over-distention of the same vessels, the general law, that action is followed by relaxation, explains how the one leads to the other.

As soon as the inordinate contraction subsides, a state of relaxation succeeds, and suffers the re-admission of red blood into those vessels from which it was before excluded; and the capillary system, before constricted, now becomes relaxed and over-distended; and this distention is for a time augmented by the force of reaction, excited in the manner explained by Dr. Cullen; namely, by the blood thrown upon the heart and larger vessels during its expulsion from the smaller.

But this cause of reaction being withdrawn as soon as the blood flows back to the capillary vessels, why, it may be asked, does not the action of the heart subside again? For symptomatic fever assumes the continued type.

The reason of this is to be found in the continued presence of the original cause of

disturbance, namely, morbid distention of the inflamed vessels. For, no sooner have the vessels partially recovered their power of resistance, than the same cause of irritation remaining, excites them to fresh efforts; these efforts again produce general participation throughout the capillary system, and thereby accelerating the return of blood to the heart, keep up active circulation; and thus inflammatory fever has no intermission.

And for the same reason that inflammatory fever has no distinct intermission, so it has no return of the cold stage; which only occurs while the vessels retain the tone and vigour of health.

But the continued struggle that is kept up does not allow even the healthy vessels to rest long enough to recover their power of action in a sufficient degree to effect another cold fit; while the state of the inflamed vessels is probably now no longer capable of exciting it.

That serious injury must be sustained by the inflamed vessels from the blood forced upon them during the general contraction of the cold stage, cannot be questioned; and

accordingly it is from this period that the full establishment of the local inflammation is commonly dated.

Now the injury sustained at this time sufficiently explains why the vessels of the inflamed part have not energy enough left to make a second effort equal to the first, or adequate to excite another cold fit.

But although inadequate to produce another cold stage, yet the partial renewal of their power, especially after the repose of the night, is sufficient to occasion a perceptible diminution of febrile heat towards morning; and thus a morning remission is commonly observable.

But as soon as the action of the heart is re-excited, the fever rises again; and as the mobility of this organ increases towards evening, this is the natural period of febrile exacerbation.

The acceleration of the pulse towards evening, noticed by various writers, has been shown, in the Essays before mentioned, to be the result of a law common to all moving organs, “*that mobility increases for a time*

with exertion ;” hence the spasmodic tendency in the limbs, from increasing mobility in the muscles, that accompanies the approach of fatigue ; hence the sense of hunger from painful contraction of the stomach, denoting increased mobility in that organ as the period of digestion is nearly completed ; and hence the acceleration of pulse, denoting increased mobility in the heart, as the diurnal period of its activity draws to a close. And thus evening is naturally the period of febrile exacerbation.

Inflammatory fever is most violent at its commencement ; and from the view which has been just given, this will naturally be the case. While the inflamed vessels still retain tone and energy enough to continue the conflict, the violence of the fever will be kept up ; but its severity will gradually decline as their power is exhausted.

With every fresh struggle their effort becomes fainter and fainter, and is followed by a further reduction of tone ; until their power being at length completely exhausted, their resistance ceases altogether ; a state of pas-

sive distention comes to prevail; the character of the inflammation changes from acute to chronic; and the fever subsides.

Such appear to be the progress of the disease, and the order of the symptoms, when the local inflammation has not been checked by timely aid, but is suffered to run its course uninterrupted.

There are, however, certain peculiarities in the character of the fever, depending upon the seat of the local affection, which call for explanation.

In fever arising from inflammation of the extremities or external organs, as in acute rheumatism, the pulse seldom rises above 90 or 96 in frequency, being full and strong; and the heat of the surface is not permanent, but interrupted by occasional perspirations. But in the fever which proceeds from inflammation of the abdominal viscera, the frequency of the pulse often rises to 120; and it becomes small and wiry, while the heat of the surface is more pungent and unremitting.

This difference in the violence of the symptoms bespeaks the presence of some circum-

stance tending to aggravate their severity in one case, which is wanting in the other.

In fever arising from external inflammation, the continuance of the hot stage was ascribed to the constant presence of the original cause of disturbance, exciting perpetually fresh resistance in the inflamed vessels, and keeping up active circulation by accelerating the return of blood to the heart.

But in fever from visceral inflammation a further circumstance appears to be super-added, which increases the permanency, and aggravates the severity of this stage ; and this is an altered irritability in the heart itself, occasioned in the following manner.

According to the laws of sympathy, as stated in Bichât's Treatise on the Membranes, parts engaged in the *same class* of functions, and deriving their powers of action from the *same source*, sympathize more strongly in each other's affections than parts engaged in functions of a *different class*, and deriving their powers of action from a *different source*. Now the abdominal viscera all belong to the same class of functions, and

accordingly show a remarkable sympathy with each other. The participation of the stomach, for instance, is seen in the affection of various organs ; thus, vomiting arises from inflammation of the intestines, of the kidneys, or of the womb ; and the morning sickness, denoting sympathetic irritability of the stomach, attends the incipient stage of pregnancy. On the other hand, the action of the heart is also liable to be altered by impressions on the stomach ; and in the same manner its mobility is morbidly increased by active inflammation of the other abdominal viscera.

In this way, then, will the severity of the fever from visceral inflammation be aggravated by a twofold cause ; namely, by accelerated return of blood to the heart, as in external inflammation ; and, moreover, in visceral inflammation, by an increased irritability of the heart itself.

The pulse now becomes quicker, and at the same time small, and hard or wiry.

It becomes smaller, because the increased irritability rendering the heart impatient of distention, causes it to contract before it is

half filled, and a smaller column of blood is thus propelled.

It becomes sharp or wiry, owing to the same morbid irritability, which causes it to contract with a smarter stroke.

And its greater frequency is accounted for, because the heart does not wait till fully distended, but contracts when half filled; thus occupying less time in each separate contraction, and leaving the venous blood still pressing on the auricles from behind.

The severity and permanency of the heat will of course be greater, as the reaction is the more violent which occasions them. And thus we may account for the difference between the two forms of fever; the reaction being kept up in fever from inflammation of the extremities, simply by the irritation in the vessels accelerating the return of blood to the heart; while in that from visceral inflammation, a sympathetic irritability of the heart itself is superadded, and the severity of the symptoms increased thereby.

VARIETIES OF FEVER.

IDIOPATHIC—INTERMITTENT.

FROM symptomatic we proceed next to idiopathic fever, which, according to the symptoms, has also been divided into different genera.

The most important distinction is that of continued, and intermittent; the latter of which we propose to make first the subject of investigation.

The phenomena of intermittent fever are peculiarly calculated to throw light on the nature of capillary action; for to this class of vessels, and to the laws by which they are governed, are the morbid changes of circulation incidental to intermittent fever clearly referable.

These changes do not appear to be occasioned in intermittent, as they are in in-

flammatory fever, by sympathy, with a local change of action; for, in this instance, we have neither evidence, from morbid dissection, of any local inflammation to occasion them, nor any indication of its presence in the symptoms attending.

The general character of intermittent fever consists in the following peculiarities:

A cold stage, more severe than in any other form of fever; followed by a hot stage, almost equally violent in degree, but short in duration; terminating in a copious perspiration; while these are succeeded by an intermission of one, two, or three days, during which the patient enjoys a state of apparent health, until the paroxysm returns.

These symptoms are wholly at variance with the existence of local inflammation as their cause.

In the first place, the total exemption from disease enjoyed during the intermission militates against this idea. But further, the extreme severity of the symptoms would require a cause equally powerful to produce them; and the local inflammation, if such be the cause, must be of the most formidable

description, to produce effects so violent. Now the more formidable the local affection, the less reconcilable with the total suspension of morbid symptoms during the intermissions.

Nor yet do the symptoms of intermittent fever appear referable to debility or loss of tone as their cause.

In no form of fever are the indications of debility less discernible than in ague; for as soon as the paroxysm is over, the patient feels his natural strength and energy, little if at all impaired; and it is only from long protraction of the disease that they appear to suffer at last.

But a further circumstance, which militates against the supposition of debility, is the violence of the cold stage; for the strength of the rigor, which consists, as we have seen, in morbid contraction of the capillary system, is always proportioned to the degree of tone that prevails; hence, in continued fever, it occurs only at the commencement, before the tone is impaired; and in ague, the rigors usually become fainter as the strength of the patient is exhausted by continuance of the disease.

The only cause that accords with the phenomena of intermittent fever, is one that is simply irritating; and accordingly we shall find in the laws of irritation a satisfactory solution of the most important of its symptoms.

In the first place, we may perceive in the circumstance of its cause being simply irritating, the reason why this form of fever is not communicable from one person to another. Intermittent does not, like some forms of idiopathic fever, proceed from a poison originally derived from the living body, and capable of assimilating with it; but from a cause incapable of assimilation, which therefore does not propagate itself, like contagion, but remains a foreign and extraneous matter, exciting continual irritation, till expelled from the system.

In the same circumstance of its acting by irritation may be found the reason why it leaves the person who has once had this disease more liable to take it again, than one who has never experienced it.

The resistance excited by this irritation is a mode of action, which is strange at first,

and not readily effected, but which naturally becomes more easy of production the oftener it is repeated; until, by frequent repetition, it becomes at length habitual, and may be excited by causes much slighter in degree, and even totally different in kind, from that which first produced it. Thus, an increased liability to the disease is accounted for, by the newly-acquired habit of action arising out of repeated resistance to a new mode of irritation.

It is even probable, when the disease is suffered to run its course uninterrupted, that the original cause of irritation is wholly expelled from the system before the spontaneous cessation of the fever takes place, and that its continuance is kept up for some time longer by this morbid association of action, as explained by Dr. Darwin.

As irritation appears to be the cause to which the phenomena of intermittent fever are referable, it is necessary to revert to the laws of irritation, in order to illustrate the operation of that cause.

When treating of the influence of corporeal impressions in producing change of function,

the effects of irritation were shown to vary according to the mode in which it is applied ; —*that which is internal to the organs exciting increased contraction ;—while that which is external diminishes or suspends it.*

Without repeating what has been elsewhere said respecting these laws, and the various corollaries arising out of them, it will be sufficient at present to recapitulate the general principles, with a few instances in illustration of each. Beginning with internal irritation, its immediate effect is to excite the organs to increased contraction, as the following examples may show.

Emetics, which, for the most part, owe their influence to the irritation they occasion, excite increased contraction in the stomach. —Purgatives, which operate, partly at least, in the same way, cause increased contraction in the intestines.—Sudorifics, which are taken into the circulation and internally applied before they act, stimulate the exhalent vessels to contract ; and their sphincters or mouths relaxing in consequence, as before stated, transpiration is increased thereby.—Secretion is in like manner augmented by certain me-

dicines received into the circulation, and applied internally to the vessels of the secreting organ.

While medicinal substances thus act by internal irritation, similar effects are also liable to result from any unusual change in the natural condition of the fluids, which renders their impression novel and irritating to the vessels that contain them.—Hence, in persons of irritable habit, in whom digestion is weak, and assimilation imperfect, a sense of chilliness is apt to occur after taking food, at that period when the chyle mixes with the blood, and excites a new impression in the vessels. The hectic flush which follows, arises from the relaxation consequent to this increased contraction, according to the general law before stated.

Such are the effects of internal irritation. The influence of that which is external, in suspending or diminishing contraction, appears in the means employed for allaying inordinate action in these organs. Thus, for instance,

In affections of the stomach or intestines,

rubefacients or blisters, externally applied, are used for this purpose. In morbid irritability of the womb, frictions and fomentations have that effect. In the vascular system the influence of external irritation is obvious and visible on the surface, in the relaxation and increased fulness of vessels produced by it, in whatever mode it is applied ;—whether mechanically, as by rubbing, scratching, or bruising ; or physically, as by the application of sinapisms, increased fulness and distention of the vessels being invariably the effect that results. And in the same way relaxation and increased fulness of the vessels is produced by strong stimulants applied to the internal surface of the stomach, where they act externally to the vessels ramified on that surface,—hence the general glow and sense of warmth that arise from taking wine or ardent spirits.

Thus the effect of irritation external to the vessels appears to be the reverse of that which is internal ; and we may now proceed to apply these principles in illustration of the effects produced by the aguish cause.

In whatever way that cause is received into

the system, whether it be through the medium of the lungs in respiration, or by impregnating the saliva and passing into the stomach, its first impression will still be external to the vessels, and relaxation along with increased circulation should therefore be the result; and accordingly it is not uncommon for an unusual flow of spirits to occur for a short time previous to a febrile attack.

Before, however, the specific effects of the irritating cause manifest themselves, absorption must have taken place; and the impression will then act internally to the vessels, and morbid contraction will be the result.

It may, however, fairly be inferred, that a certain accumulation or concentration of the exciting cause is necessary to give it effect; as few persons only take the disease out of many who are exposed to its influence.

When the dose is large enough, and its force has been sufficiently concentrated by absorption, its internal application will excite a general contraction of the capillary vessels, and thus a rigor or cold fit is produced.

The violence of this stage in ague is more severe than in any other form of fever; even

more severe than in fever from local inflammation, as might be expected. For the cause that excites inordinate contraction operates the more strongly, as it now acts by direct application, and not sympathetically; and the power of the vessels to effect this contraction is equally great, for their tone in ague, as in inflammatory fever, is unimpaired; since the cause is simply irritating, and not debilitating.

The severity of the hot stage is commensurate with that of the cold, because the subsequent relaxation bears a proportion to the previous contraction of the capillary system. And where the vessels retain their healthy tone, as in ague, each stage will manifest a considerable degree of violence. Moreover, the violence of the hot stage in ague will, for a time, be more or less aggravated by the force of reaction, excited in the manner explained by Cullen.

The duration of the cold stage is limited, because the powers of action are limited; and the more violent the contraction the sooner those powers will be exhausted. It is reasonable to suppose, that the duration of

this effort of the capillary vessels will bear some relation to that of their *ordinary* exertion; and as their state of activity usually continues somewhat more than twelve hours before relaxation succeeds; so this state of *inordinate* exertion seldom continues more than two hours before relaxation and distention begin to appear.

The duration of the hot stage is limited, because the relaxation of vessels which occasions it, arises solely from their previous overaction, and not from any permanent debility. The capillaries being fatigued only, if the expression be allowable, their power of contraction soon revives by rest. And the reaction of the heart, which for a time keeps up and increases the distention, abates as soon as the cause that excites it is withdrawn. Or, after the blood thrown upon the larger vessels during the contraction of the smaller has returned back to the small vessels, from their subsequent relaxation, the cause of reaction is removed, and its violence begins to subside. The distention of the capillaries is then moderated or becomes passive; and as they recover their contractibility by rest, the

constriction of their sphincters ceases with the distention which occasioned it, and transpiration returns.

The view thus offered will be perceived to differ from that of Dr. Cullen in the following particulars :

1st. As to the contraction of vessels in the cold fit, which is ascribed to simple irritation, and not to atony or debility.

2ndly. As to the cause of suppressed transpiration in the hot stage, which is ascribed not to constriction of the vessels, but to that of their mouths only, in consequence of the vessels being relaxed and distended.

3rdly. As to the cause of the sweating stage, the approach of which is conceived to be retarded instead of being accelerated by the violence of the reaction.

The sweating stage is followed by an interval of apparent health, the reason of which is next to be sought for.

During this stage a copious evacuation takes place internally as well as externally, which tends to promote the intermission in two ways. In the first place, by the reduction of irritability attendant upon a copious de-

pletion of the vessels ; and, secondly, by the partial expulsion of the irritating cause ; the quantity of which must be diminished with every paroxysm ; one portion being thrown off by transpiration, while another is returned back into the *primæ viæ*, where it no longer acts internally to the vessels, and therefore no longer excites them to contraction.

The paroxysm, however, recurs after a certain period ; because the portion that remains will be again taken up by absorption, and be internally applied to the vessels ; where its strength will gradually accumulate, until it acquires force enough to excite another paroxysm ; and thus the fever assumes the intermittent form.

The time required for this purpose varies from one to three days ; and the fever is accordingly denominated *quotidian*, *tertian*, or *quartan* ; and if we consider the circumstances attending each, we shall perceive the reason of this variation, and be able to account for the origin of the different types.

The circumstances in which they differ are not restricted to the length of the intermission, but comprise many other pecu-

liarities. They differ, for instance, in the length of the paroxysm, in the severity and duration of each of its stages ; and they differ also in the hour of the day at which it is liable to recur.

If all these peculiarities be found to accord with the laws of irritation, it will afford the strongest proof that the cause assigned is the true one, and that the explanation given is correct.

In order to show that all the facts accord with the principle of irritation, it is only necessary to bear in mind that the influence of irritation may be modified in two ways ; by different degrees of activity in the cause ; and by difference of susceptibility in the body. The facts before us must determine which of the two has the larger share in producing the various species of ague.

We learn from the best medical authorities that the periods of childhood and youth are most subject to the quotidian type ;—that of manhood to the tertian ;—and that of advanced age to the quartan.

Now the characteristics of childhood and youth are greater susceptibility of impression,

and greater mobility of fibre ; those of manhood are strength and vigour of fibre ; while declining susceptibility and impaired mobility characterize old age.

We are further informed, that females are more subject to the quotidian, males to the tertian and quartan types. Again, the general characteristics of the female sex are mobility and susceptibility, while tone and vigour predominate in the male.

We learn also from the same source, that persons of delicate constitution, and those who have been weakened by previous disease, are more liable to the quotidian ; and those of robust and athletic habit to the tertian and quartan types. This also coincides, as weakness induces morbid susceptibility and increases mobility.

It is also found, that the tertian and quartan types change into the quotidian, if a morbid irritability be brought on by unseasonable evacuations, or other means that impair the tone of the system, and increase the susceptibility of impression ; while, on the other hand, the quotidian may be gradually changed into the tertian, when the

*rather
Paradoxical*

tone is improved by the judicious employment of medicines.

Different degrees of susceptibility and mobility appear then to be conducive to the various species of the disease: less tone and more mobility characterizing the quotidian; healthy tone and vigour the tertian; declining susceptibility and immobility predisposing to the quartan type.

This constitutional predisposition is not, however, the sole cause, or each form of the disease would be exclusively confined to those predisposed to it; which is not the case, both sexes, every age, and every constitution, being occasionally affected with each form of the disease.

We must conclude, then, that a different degree of activity in the exciting cause may countervail the influence of predisposition; or a stronger dose of the poison may produce effects equivalent to those resulting from greater susceptibility of impression.

We shall now inquire how the symptoms accord with this view, that different degrees of susceptibility and mobility determine the different types.

Quotidian intermittent has an interval of twenty-four,—tertian of forty-eight,—and quartan of seventy-two hours.

Quotidian has the shortest intermission, because mobility being greater, morbid action is sooner or more easily excited, as it requires less concentration of the exciting cause to produce it; moreover, circulation being more active where greater mobility prevails, absorption will be quicker, and reaccumulation of the poison go on more rapidly.

Quartan has the longest intermission, because susceptibility of impression being least, the greatest concentration of the exciting cause is required to produce inordinate action. This of course takes a longer time; and moreover circulation being the slowest, absorption is less active, and thus the longest period elapses before sufficient accumulation takes place.

Tertian has an interval of intermediate length, because the activity of circulation, the mobility of fibre, and the susceptibility of impression are all intermediate.

The hour at which the paroxysm is most

liable to occur in each is also explicable upon similar principles.

The ordinary period of febrile exacerbation is evening, that being the time, as already shown, at which vascular mobility is greatest, and at which inordinate contraction is of course most readily excited.

If then the susceptibility of impression be not sufficiently great, or the cause not powerful enough to produce a paroxysm, except under favourable circumstances, that effect will not be produced until the period of greatest mobility arrives; and such being the case with quartan ague, this type has the latest paroxysm, and its return is commonly postponed till evening.

But where the susceptibility of impression is sufficiently great, or the exciting cause powerful enough to act under less favourable circumstances, its operation will not be postponed till the period of greatest mobility arrives, but may take place earlier in the day. And this being most peculiarly the case in quotidian ague, this type has the earliest paroxysm, or its attack usually occurs in the morning.

Where the circumstances predisposing to the attack are intermediate, as they appear to be in the tertian ague, the time of its recurrence will also be intermediate; accordingly this type has more commonly a noon paroxysm.

As the disease, however, is increasing or decreasing in severity, the hour of attack will be anticipated or postponed; and thus constant uniformity in this respect is not to be looked for.

None of the forms of ague are found to have a paroxysm in the night, as might be expected; for where the period most favourable to its attack has been allowed to pass over, it is not likely to occur in that which is least favourable, and this is the night; for vascular mobility and susceptibility of impression are both diminished during sleep.

The severity and duration of each stage of the paroxysm bear further testimony to the different degrees of mobility producing them.

The quotidian has the slightest and shortest cold fit;—the tertian the severest;—and the quartan the longest.

The quotidian cold fit is the slightest and

shortest, because where weakness and mobility predominate, there is least strength to effect inordinate contraction, and least constancy in the vessels to maintain it.

The tertian cold stage is most severe, because where tone predominates there is most vigour to effect, and most power to maintain this inordinate constriction.

The quartan cold fit is the longest, but less violent than the tertian; because where rigidity of fibre and immobility predominate, there is less tendency to change of action, and consequently more pertinacity in maintaining vascular constriction, though more tardiness in producing it, and less vigour to effect it.

It is true, that some authors state the quartan cold fit to be most severe; but it is probable that its longer duration, and the aversion to cold common to advanced age, only render it more intolerable to the patient.

The quotidian has the longest hot fit;—the tertian the most severe;—the quartan the shortest and least severe.

The quotidian has the longest hot fit; because this being the stage of relaxation, the

effects of inordinate exertion are most permanent where the tone is least ; or the effects of overaction are longest felt in the weakest fibre.

The tertian has the severest hot stage ; because the contraction preceding was most violent, and the relaxation that follows is proportionate. Moreover, the powers of reaction are strongest, and thus the hot stage is aggravated by a twofold cause, greater relaxation of vessels, and stronger impulse in the blood.

The quartan hot fit is shortest and least severe ; because, immobility prevailing, there was less violence in the preceding cold fit to excite the reaction, and there is also less activity in the heart to keep it up.

Thus all the phenomena of intermittent fever concur to prove that it proceeds from a cause acting by internal irritation, and exciting inordinate contraction in the capillary vessels.

VARIETIES OF FEVER.

IDIOPATHIC—CONTINUED.

FROM intermittent we proceed to continued idiopathic fever, which is also divided into different species, distinguished by the appellations of typhus mitior, and typhus gravior, synocha, and synochus.

The two former are merely different degrees of the same affection, as the name indicates. The two latter are modifications that seem, like the varieties of ague, to arise more from constitutional difference in the individual than from difference in the febrile cause.

Typhus, however, is used both as a specific and generic appellation, and in the latter acceptation may be regarded as the primary form of continued idiopathic fever.

The nature and cause of this form of fever

will be deduced from a general view of its phenomena; but it is necessary to notice here an opinion which has of late obtained many advocates,—that all fevers originate in one and the same cause, and that cause local inflammation.

This conclusion appears to be founded upon a partial view of facts, and to be irreconcilable with a general survey of the febrile phenomena.

That intermittent and inflammatory fever are perfectly distinct in their nature and cause, has, it is hoped, been sufficiently shown. And it will be rendered equally manifest, that fever of the typhoid type is essentially different from both: in fact, that it proceeds neither like inflammatory fever, from general sympathy in a local change of action; nor, like intermittent, from a cause acting by simple irritation; but from causes which are primarily and essentially debilitating.

Before we proceed to state the facts upon which this inference is founded, it is necessary to remark what is well known to every one practically conversant with diseases, that

the divisions of nosology are not always discernible. The shades of distinction are sometimes lost from the forms of disease running into each other. Thus, intermittent may assume the appearance of continued fever, from the rapid succession of its paroxysms. Or, sometimes the different forms may be blended together from a combination of the causes which produce them. Thus, inflammatory fever and typhus are frequently conjoined; and in this case the symptoms of the two are liable to be confounded.

But when the object we have in view is to investigate the nature of each, we purposely select those instances in which the characteristic features are most strongly marked; and instead of regarding symptoms which are alike common to both, we direct our attention exclusively to those which are peculiar to each.

If then with this view we select the phenomena which unequivocally belong to typhoid fever, we find its characteristic features to consist in,—the slow and insidious mode of its attack; the prostration of strength, and dejection of spirits, that mark its approach,

and attend its progress ; the comparative slightness, or total want of a cold stage preceding it ; the unremitting severity of its hot stage, attended with symptoms of peculiar malignity, such as excessive heat and thirst, great rapidity of pulse, and frequent delirium ; and besides these a vitiated state of the secretions, indicated by blackness of the tongue, foetor of the breath, and offensive alvine and urinary excretions. As the disease advances, a general putrescent tendency appears, (from which it formerly obtained the name of putrid fever), marked by a cadaverous odour exhaled from the body ; livid spots, or ulcers, running rapidly on to gangrene ; or aphthæ, accompanied by passive hæmorrhages. And to these may be added, as further distinguishing typhoid from inflammatory and intermittent fever, the circumstance of its contagious nature and origin ; with, for the most part, a diminished liability to its attack in those who have once experienced it.

Such are the peculiarities of fevers of the typhoid type ; and when contrasted with the phenomena of intermittent fever, and fever from local inflammation, they are abundantly

sufficient to establish a radical difference in the nature and cause of each.

In the circumstance of contagious fevers leaving the patient less subject to their attack, they are diametrically opposed to intermittent and inflammatory fever, both which leave him more liable to these diseases than before. A part that has been once inflamed remains weaker and more subject to inflammation than the sound parts; hence rheumatism, pleurisy, inflammation of the liver, or inflammation of the brain, leave the patient more subject to those affections than persons who have never experienced them. Ague, in like manner, leaves a stronger tendency to its recurrence, from the habit of morbid action that is generated by it. But contagious fevers, on the contrary, for the most part, leave the patient comparatively exempt from their influence.

The opposite nature of typhus, as contrasted with these two forms of fever, appears in several other peculiarities.

Simple irritation, which operates by exciting inordinate contraction, can only act periodically, for reasons formerly alleged;

and therefore produces a fever which is intermittent. But the febrile symptoms in typhus are unremitting, and denote therefore a cause which is permanent.

Fever from local inflammation is most violent at its commencement, the inflammatory symptoms declining in severity as the general tone is reduced by depletion, or impaired by protraction of the disease. Typhus, on the contrary, steals on by slow degrees, being generally slight at first, the typhoid symptoms increasing in severity the more the powers of life are exhausted.

Inflammatory fever, like ague, is preceded by a severe cold stage; and the greater the tone of the vessels, the stronger the rigor produced. The attack of typhus, on the other hand, is preceded only by slight chills at most; and it may fairly be questioned, whether a distinct rigor ever belongs to genuine typhus, when unconnected with local inflammation. Thus Sydenham, who particularly notices the rigors that precede those fevers which are evidently combined with local affection, such as rheumatism, pleurisy, measles, small-pox, and plague, makes no

mention of the rigors preceding putrid fever. Aretæus, in his memorable description of burning fever, says nothing of rigors preceding its attack; and Lommius, celebrated for his accuracy of delineation, in treating of putrid and continued fever, says,—“*porrò continuam febrem neque horror aliquis, neque rigor, neque frigus antecedit.*”—Huxham indeed says, that the rigors in putrid fever, *if any*, are severe. But can we suppose that a difference so great, as a severe cold stage, and no cold stage at all, is characteristic of one and the same affection, or bespeaks no difference in the nature of the disease? Is it not more reasonable to conclude, that local affection, as so frequently happens, is conjoined with the typhus in one case and not in the other?

But the characteristic features of typhus are all wanting, both in fever from simple irritation, as in ague, and also in fever from local inflammation; such as the great prostration of strength, the frequent delirium, the vitiated secretions, the dark fur on the tongue, and the putrescent tendency: and thus the phenomena no way accord with those

of fever arising from simple irritation or local inflammation.

The only cause that coincides with, and is adequate to account for the phenomena of typhus, is one that is primarily and essentially debilitating: a cause that owes its influence, not to the power of irritating, and thus exciting inordinate action, but to its property of assimilating with and becoming part of the body; either destroying, or for a time impairing its power of action, and often permanently altering the condition of the solids or fluids.

That typhoid fever proceeds from debility may in the first place be inferred from the circumstances which predispose to its attack being all of a debilitating nature; such as unwholesome diet, depressing passions, confined air, want of cleanliness, the abuse of intoxicating liquors, or any habitual excess, which by impairing the tone and vigour of the body, renders it less able to resist the noxious influence of a cause prejudicial to life, as contagion evidently is.

That the contagion of typhus owes its in-

fluence to the property of weakening and undermining the powers of life, next appears from the symptoms ; which all indicate vascular debility as their cause, and on this view admit of explanation.

A well marked rigor, as before noticed, is not characteristic of this form of fever.

Genuine typhus, indeed, may be comparatively rare. In the epidemic which has of late prevailed, perhaps not one case out of ten (See Dr. Crawford's Inaug. Dissert. de Febre Epid. Edinb. 1820.) is wholly divested of local affection ; for the insidious mode of its attack allows the patient to follow his ordinary vocation, until some one organ gives way under the general weakness that prevails ; and thus local inflammation is frequently conjoined with typhus, and the attention is often first called to the local affection. But genuine typhus without local inflammation has little or no cold fit, never a strong rigor preceding its attack ; as Lommius, Sydenham, and Aretæus attest.

The reason of this appears to be twofold ; first, since it does not proceed from a cause

calculated to excite a rigor; and, secondly, because the vessels are not in a state fit to effect one.

The rigor consists in an effort of inordinate contraction, which is excited either by sympathy or direct irritation; but in pure typhus we have no local inflammation to call forth sympathy, and the cause of typhus assimilates and does not irritate, and therefore is not calculated to excite this resistance.

A degree of tone and vigour is requisite for effecting a strong rigor; but the cause of typhus weakens and impairs the tone of the vessels, and thus unfits them for effecting it.

The chilliness which sometimes precedes typhus, accompanied by languor and debility, is widely different from the strong rigors of ague; and denotes rather diminished impulse in the blood than increased resistance in the vessels.

In fact, this stage in typhus may be truly regarded as the state of atony and debility noticed by Cullen: a state which perfectly accords with the symptoms of typhus, though it is not consonant with those of inflammatory fever or ague. In these the rigor is strong

and the debility wanting ; whereas in typhus the debility is extreme, and the rigor slight or wanting.

The essential characteristics of typhus are the permanency and severity of its hot stage, attended with the malignant characters before enumerated.

The hot stage of typhus bears the closest affinity to a state of local inflammation, and may be regarded as a general inflammation. But although typhus bears a close analogy to local inflammation, yet a material difference prevails between typhoid and inflammatory fever.

Inflammation and typhus both proceed from weakness and distention of vessels, local in one case, general in the other ; but inflammatory fever derives its chief peculiarity from the general tone of the vessels being unimpaired, while the debility is local ; whereas in typhus the debility is universal. And this circumstance is most important, as it explains, in the first place, why typhus has no distinct rigor ; and further, why the attack of typhus is slow and insidious, while that of inflammatory fever is sudden and violent.

Neither typhoid nor inflammatory action immediately succeed the application of the noxious cause which gives rise to them; as debility alone does not constitute disease. Vascular distention is alike the leading feature in both; but as the vessels have no power of distending themselves, some cause of distention must be applied before the effects of debility can become manifest in this way.

When the debility is local, as in topical inflammation, the ordinary impulse of the blood is sufficient for producing distention; and thus inflammation soon takes place after the local injury, and inflammatory fever as quickly succeeds.

When the debility is universal, as in typhus, the heart also participating, the impulse of the blood is impaired, and the distending cause thus in part withdrawn. The fever therefore is not so soon formed; or, the state of debility may go on imperceptibly increasing, until some accidental cause excites active circulation, produces vascular distention, and thereby establishes the fever.

The affinity which the state of the vessels

in typhus bears *generally* to that which prevails *locally* in topical inflammation, appears in the identity of the cause which produces that state in each, in its permanency, and in its severity.

The cause which produces each is debility, and its mode of operation, according to the explanation suggested by John Hunter, appears to be as follows. Debility occasions distention, over-distention excites resistance, and this resistance or struggle, when ineffectual in removing distention, produces increased circulation, which is attendant alike upon febrile and inflammatory action.

The permanency of the distention in typhus is uninterrupted, as it is in local inflammation; because the debility is permanent which occasions both. The degree of it is extreme, because the distention is aggravated in each by a two-fold cause, as soon as the fever is fully established; namely, diminished resistance in the vessels on the one hand, and increased impulse of the blood on the other.

The inordinate frequency of the pulse speaks increased impulse of the blood in typhus, and the uninterrupted continuance

of that frequency cannot be otherwise accounted for than by an altered irritability of the heart, as formerly suggested. Or, as the muscular fibres of the stomach, intestines, and all other parts, acquire an increase of irritability from augmented circulation in their muscular structure, so is the irritability of the heart also increased by augmented circulation in its muscular structure. And the cause of typhus being transfused through the blood, and applied to all parts, there is no reason why the heart should be exempt from its influence. On the contrary, its participation is evinced in both stages; namely, in the first instance by oppressed circulation, denoting impaired irritability in this organ during the stage of languor and depression; and afterwards by inordinate frequency of the pulse, denoting morbidly increased irritability in the heart.

The conjunction of augmented irritability in the heart with increased vascular action was before noticed as an aggravating cause of the fever arising from visceral inflammation. But the effect is not exactly similar in both these instances. For while the mo-

bility of the heart is alike augmented in both, the tone of the vessels is not impaired in both. On the contrary, the contractile power of the vessels is undiminished in fever from visceral inflammation, the debility being local; and hence the sharp, wiry feel, peculiar to the pulse in this form of fever, is not an attendant upon typhus, in which the vascular debility is general.

The other indications of general loss of tone in typhus are still less equivocal. The delirium, which is more frequent in this than in any other form of fever, bespeaks loss of tone in the cerebral vessels. The dark fur on the tongue, and the offensive evacuations from the intestines and bladder, bespeak loss of tone in the secreting organs. The livid spots on the surface show the admission of red blood into vessels which exclude it at other times, but now both admit and allow it to stagnate for want of tone and energy to propel it onwards. The frequency of passive hæmorrhages affords further indication of loss of tone, extending to the mouths of vessels and to the excretory ducts; while the broken down and dissolved state of the blood, no-

ticed by Huxham and others, contributes to produce the hæmorrhagic tendency. And, lastly, the cadaverous odour exhaled from the body, and the disposition to gangrene, bespeak the extreme of debility, or the almost total extinction of the powers of life.

Such are the symptoms peculiar to fevers of the typhoid type; all which denote the operation of a cause that acts neither by vascular sympathy nor by simple irritation, but by weakening and destroying the tone of the vessels.

Thus the primary causes of fever seem reducible to three:—

First, Local inflammation, which acts by vascular sympathy; a change of action in the injured vessels exciting participation in those which are sound;—

Secondly, Direct irritation, which excites inordinate contraction only when internal to the vessels, and thus acts periodically, producing intermittent fever; and,—

Thirdly, Primary debility, which disposes the vessels to a state of morbid distention whenever an accidental cause occurs to excite increased circulation: such appears to

be the nature of fevers proceeding from contagion.

In conformity with the view which has been given of the nature of typhus, and the operation of the cause which produces it, we may form a probable conjecture why contagious diseases often leave the patient less subject to their attack, while ague always leaves him more so than before.

The increased susceptibility that remains after ague, and the non-contagious nature of this affection, appear to be connected together, or to be correlative effects proceeding from the same cause, as partly explained before ; namely, that the aguish cause does not produce a contagious disease, because it is incapable of assimilating with and reproducing itself in the body, and therefore remains a foreign and extraneous matter, exciting continually fresh irritation until it is wholly expelled from the system by the resistance it occasions ; while the habit of morbid action which it generates remains behind and is easily renewed, and thus the patient becomes more subject to attacks of the same disease.

So, on the other hand, the property which contagious matter possesses of reproducing itself, with the diminished susceptibility it leaves, appear to be also connected, as correlative effects resulting from the same cause; as follows—

The matter of contagion is derived from the body, and being therefore homogeneous, does not irritate, but readily assimilates with the body, multiplying and reproducing itself, and thus begets a contagious disease. While, for this reason, or from its property of assimilating with the body, it occasions certain changes, which either extinguish the powers of life, or leave some permanent alteration in the condition of the fluids or solids, rendering them less susceptible of again undergoing the same changes from the same cause; and thus a partial exemption from its influence follows its attack.

In short, it may be said, that the cause of ague irritates because it will not assimilate, while the cause of typhus assimilates, and therefore does not irritate.

Although no particular regard has been

paid in the present inquiry to nosological arrangement, yet it is not surprising that the arrangement arising out of the nature of fever should present a division of its varieties into orders, genera, and species, the same as that which is founded on external character, since both originally spring from the same source.

The varieties of continued, like those of intermittent idiopathic fever, will be found to depend partly upon constitutional peculiarity in the individual affected, and partly upon original difference in the febrile cause.

Thus typhus mitior, or typhus gravior is known to prevail sometimes epidemically, presenting nearly the same appearance in all, whatever be the constitution of the patient. But, on the other hand, it is allowed by most medical writers, that the character of continued fever is subject to material modification from constitutional peculiarity, presenting one form where the inflammatory diathesis prevails, and another where the habit of body is the reverse of this.

Accordingly, the inflammatory character, or that of synocha, is more common amongst

the robust peasantry of the country, amongst the active and laborious, and amongst those who are in the prime of life, and were previously in full health and vigour; while the typhoid character is more common amongst the inhabitants of large towns, who breathe a less invigorating atmosphere, amongst those who are employed in sedentary occupations, and those whose constitution may in any way have been previously debilitated.

How this difference tends to modify the form of fever proceeding from a cause which acts by debility, may be easily shown; but it is not easy to conceive that any habit of body can so modify the effects of a debilitating cause, as without local inflammation to produce a purely inflammatory fever; the essential attribute of which consists in the general tone of the vessels being unimpaired while the debility is local. Nor is it probable that the opposite state of general debility will often prevail to such a degree as to produce pure typhus, wholly unattended with any marks of constitutional vigour in its first stage.

Accordingly, many authors doubt the exist-

ence of pure unmixed typhus ; and still more call in question the occurrence of perfect synocha unattended with local inflammation.

And what is most consonant with the view that has been offered, is confirmed by experience ; that the most usual form of continued fever is the intermediate one termed synochus ; which exhibits more or less of the inflammatory character at the commencement, and becomes typhoid towards its decline, if left to run its course uninterruptedly.

To explain how the character of the disease may be modified by the mode of life and previous habit of body, we have only to reflect, that debility alone does not constitute fever ; but that some accessory cause is required to increase circulation, and produce vascular distention before the fever can be fully established.

Now whatever be the febrile cause, whether it be contagious or otherwise, if its influence be debilitating, we may reasonably conclude, that its effect will be greater the longer it has preyed upon the constitution before the fever is excited ; and, on the contrary, the less will be the previous debility, the earlier the

fever is excited after exposure to the debilitating cause.

But we are told that typhus is slow and insidious in its attack, while synocha is sudden and violent; which, in fact, amounts exactly to this, that where the cause is allowed to remain longer in the system before the fever is excited, and thus more effectually to undermine the constitution, the fever assumes the typhoid character;—but when it remains a shorter time, and does less previous injury, the fever exhibits more of the inflammatory character, or the form of synocha.

Or, if properly stated, the fever is not slow in its attack because it is typhoid; but becomes typhoid in consequence of the slow and gradual manner of its approach. So in synocha, the suddenness of the attack does not proceed from the nature of the fever; but rather the inflammatory character of the fever from the suddenness of the attack.

Accordingly the active and laborious will be most subject to the inflammatory form, as their continual exposure to the accessory causes will occasion the fever to be soonest excited in them; while the sedentary and

the indolent will be more liable to the typhoid type, as in them the febrile cause will be allowed to remain longer before the exciting cause is applied.

It has been remarked of the late epidemic, that the lower class were more subject to its attack than the higher; which may in part be ascribed to their being more strongly predisposed to it, owing to their suffering more from the pressure of want. But it was also observed, that the fever proved more fatal when it occurred among the higher class than among the lower; which is explicable upon the principle before stated; namely, that being less exposed to the accessory causes of fever, a longer period would be allowed to elapse with them, and thus greater injury be sustained before the fever was fully formed.

The combination of typhus with local inflammation is not strictly a modification of idiopathic fever, as the inflammatory stage in this case is sympathetic, and proceeds from the local affection. Nor is the fever arising from certain specific contagions, which act partially in the first instance, and produce local inflammation, as small-pox, to be re-

garded as idiopathic, being in reality compound or sympathetic in the first stage, and idiopathic in the last. Accordingly, the severity of the inflammatory stage coincides with the extent of the eruption; and this being checked by cold, and increased by heat, the fever is rendered mild or severe in proportion.

The influence of heat in aggravating the severity of fever extends to every form of this affection, and seems to operate both on the animal frame and on the febrile cause. Insomuch, that the same cause which produces a mild form of fever in a temperate climate, may occasion a more virulent form in a hot one; the same noxious exhalation that produces an intermittent in a cold, may give rise to a remittent fever in a hot country.

From which some contend, that all fevers are essentially the same, and that all spring from the same cause. But this inference is unwarranted.

If a tertian change into a quotidian, this was shown to arise from the influence of debilitating causes, increasing susceptibility, and thereby hastening the return of the

paroxysm ; so if an intermittent degenerate into a remittent, or a remittent into continued fever, it equally proceeds from the same cause; and thus the principle laid down still holds good, or debility is the cause that disposes to the continued type, while the regular intermittent proceeds from simple irritation.

The influence of excessive heat on the animal frame is known to be relaxing and debilitating, and therefore disposes the febrile paroxysm to a more quick return ; and in this way the cause that produces a quotidian intermittent in a cold country, may produce a remittent fever in a hot one. But the influence of climate operating also on the febrile cause may not only increase the virulence of the noxious exhalations conceived to excite the fever, but may render them debilitating as well as irritating ; and it is probably to this combined influence that the remittent fever prevalent beyond the Atlantic owes its origin ; having besides another peculiarity, clearly referrible to the influence of climate, which is the biliary derangement.

When a disease is communicated through the medium of the air, as is the case with

contagious fever, it is not improbable that peculiar states of the atmosphere may both increase the activity of the cause, and render the body more susceptible of its influence. And a contagious disease may thus become epidemic; as experience shows to happen frequently with typhus.

When a disease is communicable by contact only, as is the case with the Levant plague, the influence of climate may still operate in both these ways; that is, by altering the activity of the morbid poison, as well as the aptitude of the surface to receive its influence; and thus may this affection also assume the character of an epidemic at particular times and seasons. In fact, it appears that the extremes of heat and cold are alike capable of repressing its activity: the one probably by greatly increasing cutaneous transpiration, and the other by almost suspending it.

A circumstance deserving of notice, as it confirms the view that has been given of typhoid fever arising from loss of contractile power in the capillary vessels, is the sudden and extraordinary emaciation that accom-

panies convalescence from a typhoid attack. This change seems to denote the restitution of tone, and renewal of contractility in the vessels.

That the emaciation in question does not depend upon causes which are gradual and progressive, such as depletion or inanition, appears from the suddenness with which it takes place ; being often the effect of a single night. And, on the other hand, the adequacy of an altered condition of the capillary vessels, with a restoration of their tone and contractility, to account for the change, and produce this emaciation, is shown in the different states of the body during the hot and cold stage of an ague ; the latter producing a general shrinking and attenuation, while a general fulness and apparent increase of bulk attend the former.

But the affinity which the shrinking with decrease of bulk, arising from renewal of contractility in the vessels on convalescence, bears to the over-contraction of these vessels in the cold stage before their tone was lost, appears from their liability to over-exert this power on its revival, and thus produce a

return of the rigor or cold stage, which not unfrequently attends the recovery from a febrile attack.

No obvious reason indeed appears why the recovery from pure typhus should produce a rigor or cold stage, when the original attack does not ; nor is this probably the case unless where the unequal restitution of tone, or some local cause, gives rise to a partial congestion.

Accordingly this rigor is not an usual occurrence, and when it does happen, is not always succeeded by a salutary change ; the result probably depending upon the degree of the local congestion, and the violence of the rigor. If slight, a recovery may follow ; if severe, a relapse may be apprehended ; and accordingly Dr. Cheyne remarks, in the Dublin Hospital Reports, that he found it often difficult to distinguish the rigor of solution from the rigor of relapse ; and Sydenham notices this return of a cold stage after continued fever, as a circumstance that excited alarm in his mind, until experience had taught him that it frequently denoted a salutary change.

PRECEDING VIEWS CONFIRMED BY
THE REMEDIES USED IN FEVER.

THE different nature of the three forms of fever which have been pointed out as arising from sympathy, irritation, or debility, admits of further proof from the opposite means required for the cure of each; an argument which could not have been urged with due weight until the means of contrasting the remedies were afforded by the previous elucidation of the nature of each form of the disease.

As the doctrines of fever which have hitherto prevailed appear objectionable more on the ground of their indiscriminate application, than their want of truth individually; so the danger of adopting them arises from the promiscuous practice which is likely to result from views that are partial and imperfect.

So long as the mind is possessed with the

idea that all fever originates in local inflammation on the one hand, or in general debility on the other ; it is impossible that the practice should be uninfluenced by one or the other of these impressions.

And the extent to which bleeding has at one time been carried, and the exhibition of bark at another, shows the reality of this danger ; and the only permanent security against the recurrence of such errors is to be found in more enlarged and more accurate views of the pathology of fever.

The survey here proposed to be taken of the curative means and their mode of operation, must needs be general, as the subject is too extensive to admit of our entering minutely into this question.

The causes of fever have been reduced to three, sympathy, irritation, and debility ; and the means required in each bespeak the difference of their nature.

Sympathetic fever derives its chief peculiarity from the circumstance that the general tone of the vessels is unimpaired, while the cause which excites a morbid change of action is local.

The increased circulation, or the reaction which results from this general participation in a local change of action, constitutes the leading characteristic of this form of fever, and proves at the same time the most formidable aggravating cause of the local affection.

The primary object of the cure therefore consists in reducing the force of circulation by bleeding and other modes of depletion. Indeed such is the efficacy of depletion, that it may be almost deemed a specific, for this form of fever ; which is at once subdued by it, while the cure of the local affection is greatly promoted by the removal of this aggravating cause.

But idiopathic continued fever, and especially typhus, proceeds from weakness in the vessels, more than increased impulse in the blood ; and the effects of depletion are therefore less decisive, as debility may still remain, and keep up vascular distention, after the impulse of the blood has abated, or been reduced by depletion.

But though no longer a specific, bleeding may still be beneficial ; and as vascular dis-

tention is the predominant feature, removal of distention must needs be promoted by relieving the vessels of part of the load that oppresses them. And therefore depletion, especially in synocha and synochus, and at the commencement even of pure typhus, is often salutary.

Moreover, the frequent combination of local affection with this form of fever often renders the employment of the lancet indispensable ; and where no local affection attends, still, more or less of the inflammatory character usually prevails at the commencement of the attack, and calls for its employment.

In the more advanced stage of typhus, when debility becomes decidedly the predominant feature, the restoration of tone calls for the adoption of means widely different from those which are applicable to the cure of inflammatory fever, such as tonics and cordials, and thus bespeaks their opposite nature.

But a remedy which more strikingly displays the nature of typhus as arising from debility, is one which often cuts short this

disease at its commencement, and at once effects its cure, but which is wholly inadmissible where a tendency to local inflammation appears, and this is the affusion of cold water.

The operation of this remedy is no way ambiguous. The constrictive influence of cold braces up and contracts the distended vessels; and as the heat and dryness of the surface were shown to arise from over-distention of the vessels, exciting a spastic stricture of their mouths or sphincters, so the removal of distention allows the pores to relax, and thus restores transpiration.

Transpiration may be promoted by other means also which remove vascular distention; and amongst these one of the most conspicuous perhaps is blood-letting. The efficacy of this remedy in reducing febrile heat is attested by Dr. Currie, in the Medical Reports; where he states, that the heat fell by the thermometer as the blood flowed from the arm. But since different causes produce this distention in each form of fever, so different remedies are eligible in each for its removal: bleeding in inflammatory fever,

where it is chiefly kept up by the force of reaction ; cold affusion in pure typhus, where vascular debility is its primary cause.

As it does not belong to the province of the pathologist to lay down minute and particular rules for practice, but merely to illustrate the nature of disease by showing the opposite remedies that are applicable to its different modifications, so we are not called to dwell upon the means that are alike proper in all its varieties, such as purgatives, sudorifics, and emetics.

The third form of fever, or intermittent, calls for a plan of treatment widely different from that required in either of the former ; and the peculiarity of its nature is strikingly displayed in the remedies by which it is removed.

Irritation internal to the capillary vessels, exciting inordinate contraction, was shown to be the cause of the febrile paroxysm, and all the remedies will be found calculated to prevent this morbid contraction from taking place.

The means employed are indeed very dissimilar, and when various remedies are found

to cure the same disease, it by no means follows that they must all act on the same principle.

But as the aguish paroxysm proceeds from an internal irritation, so most of the remedies will be found to excite an external or counter-irritation, thereby preventing this contraction from taking place ; by a stronger effacing or obscuring a weaker impression, according to an aphorism of Hippocrates.

The most conspicuous quality in bitters and tonics, to which class the principal remedies in ague belong, is the property they possess of exciting a powerful and durable impression when applied on the internal surface ; where they must act externally to the vessels ramified upon that surface. And in order to give them full effect, as large a dose as the stomach will bear should be administered a short time before the paroxysm is expected.

The reason of this is obvious. In order that the internal irritation may be effectually superseded, it is requisite that the external or counter-irritation should be simultaneous, and as strong as possible.

To this principle we may reasonably refer the operation of the vegetable bitters; and the metallic tonics, to all appearance, act in the same way. Thus arsenic has been recently proved by experiment to exert a remarkable action on the surface of the stomach, so as even to erode the mucous membrane if too freely administered. And sulphat of zinc likewise excites a very powerful impression, and given in doses too small to produce vomiting, is also found to stop the paroxysm of ague.

Opium, which is also a valuable remedy in ague, seems to act in a different way. This drug appears to be directly sedative or relaxing, and the power it possesses of deadening sensibility, and thus allaying inordinate action, is sufficient to account for its influence in preventing morbid contraction of the vessels. Instead of exciting a counter-irritation, it rather appears to render the vessels insensible to the irritation which is present, and thus prevents the resistance which would otherwise be produced by it.

In confirmation of this view, it may be stated, that the relaxing influence of opium

is much increased by the exhibition of an emetic immediately preceding it. The sudden constriction of vessels produced by an emetic, shown by the paleness that attends vomiting, disposes the vessels to subsequent relaxation, according to the general law before stated, and thus favours the relaxing influence of opium.

The benefit of emetics in ague is not, however, confined to this. The sudden change of impression they occasion, and the new mode of action they call forth, are well adapted to prevent the morbid habit from being confirmed, which frequent repetition of the paroxysm is found to generate.

Another remedy, more remarkable perhaps for its singularity than its utility, but which is said however to have been employed with success in ague, is, the application of a tourniquet, at the commencement of the cold stage, round the arm of one side and the thigh of the other. The operation of this remedy, which appears to be purely mechanical, singularly illustrates the nature of the cold fit, as consisting in capillary constriction. It arrests the blood in the ves-

sels, and prevents inordinate contraction by forcibly distending them, and thereby checks the formation of a new mode of action.

To conclude, if we review the remedies appropriate to the different forms of fever, the diversity of their nature will perhaps appear more conspicuous from the means which are not, than from those which are, applicable to each.

Thus bleeding, which is almost specific in inflammatory fever, is rarely called for, and generally inadmissible in ague. Cold affusion, which often cuts short an attack of typhus, would be dangerous, if not fatal, in fever from local inflammation. Bark, arsenic, and opium, if administered in the same manner as in ague, would be injurious in typhus, and wholly inadmissible in every form of fever with local inflammation.

From this the danger of promiscuous practice in fever cannot fail to appear, and the advantage of more enlarged and more correct views of its pathology must be equally apparent.

SUPPLEMENT.

AFTER the writer has allowed in his preface that one of the legitimate objects of experiment is to verify the truth of principles previously deduced, it is but reasonable to expect that his own will be submitted to this test.

Admitting then the authority of the tribunal before which he may expect to be summoned, he prefers pleading his own cause to the chance of leaving it in the hands of an advocate less interested in the success of his suit.

He is perhaps the more disposed to undertake this task from a conviction that it will afford confirmation of the correctness of his views, and that sufficient reason will be found to invalidate any conclusions that are hostile to them.

The experiments to which he proposes to call the attention of the reader, are those which have been lately made by M. Le Gallois and Dr. Wilson Phillip; and some formerly made by Mr. Brodie. Their importance, the publicity they have obtained, and the deservedly high reputation of the gentlemen who performed them, are ample reasons to vindicate this selection.

The object proposed in examining them is not to inquire into the views founded upon them by others, but to ascertain whether they present any thing that militates against those contained in the preceding pages.

Far from this being the case, it will be found, on the contrary, that they confirm the truth of these principles; which are, moreover, capable of reconciling differences in the results of the experiments apparently contradictory, and adequate to afford the solution of paradoxes arising out of them, which appear otherwise inexplicable.

However various were the objects with which these experiments were undertaken, and however different the conclusions drawn from them, the writer believes it will be found

that the most material facts they present are all explicable on one common principle. The explanation proposed will therefore be much facilitated by the previous exposition of that principle.

This is the general consent of action that prevails throughout the capillary system, in consequence of which any considerable local change of circulation soon becomes general; and the subjection of this system to impressions made upon the nerves, by which the contraction of these vessels is liable to be suddenly increased or diminished; an instance of which is seen in the general and death-like paleness produced by any violent shock or impression on the nerves. To this cause, it has been shown, in the Essays before alluded to, not only that corporeal impressions acting locally owe their power of exciting general participation, but likewise that this is the only satisfactory mode of accounting for the influence of mental impressions, or the operation of the passions, acting primarily on the circulation of the brain.

With respect to the nature of this sympathy, it is only necessary to bear in mind

that it resides in the vascular system ; and if ultimately dependant upon nervous influence, it is to the nerves entering into the texture of the vessels, and not to those subservient to voluntary motion, that this dependance appears referable.

The brain, if not essential to this subjection of vessels to nervous influence, and their general participation in a local change of action, is of all organs most conducive to its support ; and those parts directly dependant upon the sensorium are most sensible to its effects ; as might be expected, since it is the peculiar office of this organ to receive external impressions, and communicate their influence to distant parts.

On the other hand, the heart seems to be of all organs most exempt from participation in external impressions, which might often prove fatal by disturbing the regularity of its function ; and it accordingly appears to experience their influence only in a secondary way, or from the quantity of blood sent to it being increased or diminished, by the altered state of the capillary vessels.

The changes in the capillary system, arising

from the general sympathy of these vessels with the part primarily acted upon, appear to be limited to two states ; or their condition is either altered to that of increased contraction, or to that of direct relaxation, according to the nature of the local impression.

But as this general participation results from cerebral influence, and as all parts are not equally subject to that influence, it is reasonable to expect that it will not be communicated with equal promptitude to all ; and thus the effects may in some degree vary according to the seat of the local impression ; but in general the effects of cerebral influence will be most sensibly felt in the external surface and voluntary organs, while other parts will experience them more slowly and less sensibly.

Thus if the primary effect of any shock or sudden impression on the nerves be increased contraction of the capillary system, such as appears to result from the emotion of fear, blood will be expelled from the brain and voluntary organs, and congestion will be produced in the heart, and probably in the lungs.

If, on the contrary, the immediate effect be

relaxation of the capillary vessels, a change which appears to attend the emotions of joy and anger, the blood will be determined more freely to the brain and surface, and partially withdrawn from the heart and lungs.

When either of these changes is sudden and extreme, as shown in the influence of the most powerful emotions of the mind, such as rage or terror, instant death may be the consequence.

Thus, when the capillary constriction is excessive, syncope may arise from the brain being deprived of blood, while the heart and lungs may be overwhelmed by the quantity thrown upon them. In this case syncope will be the immediate precursor of death, and after death the heart and lungs should bear marks of congestion; and the former will probably be found to have lost its power of action.

When, on the other hand, the relaxation of these vessels is sudden and excessive, the opposite effect will result; and as the brain receives the blood coming from the heart sooner and with more force than other parts, this organ is most likely to be the seat of con-

gestion, from the resistance of its vessels being withdrawn while the impulse of the blood sent to them is for a time at least unimpaired, if not even augmented. Convulsions will now most probably be the precursor of death, and afterwards the brain should bear marks of congestion, while the heart will be found free from it, and retaining its irritability.

Now there is no reason why the same inordinate effects should not be produced by strong corporeal as well as by mental impressions; and accordingly Mr. Brodie was struck with the coincidence between the effects of the passions and those presented by the experiments he performed with narcotic poisons.

These experiments were made for the purpose of ascertaining the mode in which sudden death is produced by certain narcotic poisons, and are related in the Philosophical Transactions for 1811. Mr. Brodie also made experiments on other poisons, which appear to operate in a different manner; but these it does not fall within our purpose to notice at present.

In those which are here alluded to, death

appeared to be produced either in the one or the other of the modes already stated ; as a few examples will serve to show, without detailing the whole of them.

As an instance of those producing sudden relaxation and congestion in the capillaries of the brain and cerebral system, we may take exp. 5 on the oil of bitter almonds, related as follows :

“ One drop of the essential oil of bitter almonds was applied to the tongue of a young cat. She was instantly seized with convulsions ; then lay on one side motionless, insensible, breathing in a hurried manner ; the respirations became laboured, took place at longer and longer intervals, and at the end of five minutes from the application of the poison, had entirely ceased, and the animal was apparently dead ; but on opening the thorax, the heart was found acting regularly eighty times in a minute, circulating dark-coloured blood, and it continued to act for six or seven minutes afterwards.”

Similar effects were produced by proof spirits injected into the stomach ; by the juice of the leaves of the aconite ; by the

empyreumatic oil of tobacco ; and by the woorara in powder, or in paste, applied to a wound.

No particular morbid appearances were detected in the brain, but Mr. Brodie notices the close resemblance which these symptoms bear to those attending injury of the brain ; and as the heart was found still acting, he ascribes the fatal effect to the influence of the poison on the brain.

The view that has been suggested leads also to the same conclusion, although it explains in a different manner the nature of the change produced, or the way in which that organ is affected, ascribing it to nervous influence altering vascular action.

Mr. Brodie, on endeavouring to ascertain its mode of action, found that the effect of the woorara applied to a wound, which acted in a similar manner to the oil of bitter almonds, was not intercepted by tying the thoracic duct, and thence justly concluded, that it did not act by absorption.

He also found, that this poison was not prevented from taking effect by dividing the nerves of the limb to which it was applied.

From which it appears that these nerves are not the medium of communication.

Lastly, Mr. Brodie found that the effect *was* intercepted by a ligature round the limb, excluding the nerves, but including the vessels, large as well as small. From this Mr. Brodie concludes, that it must act by entering the divided veins.

But a ligature might serve to check the communication of altered capillary action as well as to prevent the transmission of the poison; as a tourniquet round the limbs stops the cold fit of an ague. And in many cases the effect could not be ascribed to transmission of the poison, being too sudden for this to take place; while in other cases no veins were divided, as in that with the oil of bitter almonds.

While the poisons of this class appeared to act by instantaneously suspending capillary action, producing relaxation of vessels, and inordinate congestion in the sensorium; those of another class appeared to act in the opposite way, or by exciting inordinate contraction in the capillary system.

To this class belong the watery infusion of

tobacco, and the upas antiar. The effects of the tobacco are related as follows. Exp. 8.

“ Four ounces of infusion of tobacco were injected into the rectum of a dog.

“ Four minutes afterwards he retched, but did not vomit; he then became faint, and lay motionless on one side; at the end of nine minutes from the time of injection, the heart could not be felt, he gasped for breath at long intervals; and in another minute there was no appearance whatever of life.

“ I immediately laid open the cavities of the thorax and abdomen. The heart was much distended, and had entirely ceased to contract; there was no peristaltic motion of the intestines.”

From this and similar experiments Mr. Brodie concludes,—“ that the effect of the infusion of tobacco, when injected into the intestines of a living animal, is to destroy the action of the heart, stopping the circulation of the blood, and producing *syncope*.”

In this conclusion we likewise concur, at the same time accounting for that effect also

in a different manner from Mr. Brodie, as already explained.

Mr. Brodie afterwards observes, that the heart in one instance resumed its action on division of the pericardium. And he further observes, that tobacco destroys also the function of the brain, which did not return, although circulation was restored in one experiment, and kept up by artificial respiration.

From which it appears, that the action of this poison was not exclusively confined to the heart, but affected also, and not less powerfully, the brain, and likewise the intestines. In short, it appears probable that it affects all parts in the same way, or through capillary constriction impeding circulation, and impairing irritability; while the heart and probably the lungs are oppressed by the quantity of blood driven from other parts and forced upon them. Accordingly the heart was found gorged and distended with blood, and deprived of irritability.

As we consider the brain chiefly instrumental to the production of this general

change of capillary action, we may now inquire what was the effect of removing this organ previous to the application of the poison.

When Mr. Brodie injected the infusion of tobacco into the intestines of a dog, whose head was cut off, and whose circulation was maintained by artificial respiration, very different was the effect produced.

The pulse now rose instead of sinking; the peristaltic action was increased instead of being diminished; the voluntary muscles were thrown into violent and spasmodic action, instead of being paralyzed. In short, the effects were the very reverse of those produced before.

Thus it appears that the co-operation of the brain is necessary to produce the specific effect of the poison, as its removal prevents that effect.

But although removal of the brain may prevent this effect from taking place, how can we account for its producing the very opposite results?

To account for this, we require to know how much of the effect, in the case of de-

capitation, is due to the influence of the poison, and how much to accessory circumstances.

If the poison, when operating through cerebral influence, cause capillary constriction, and by thus impeding circulation diminish irritability ;—removal of the brain may explain the prevention of this effect ; or why capillary constriction no longer occurs, when the means of constriction or cerebral influence is removed. But to account for the opposite state, or positive distention of vessels, and increased circulation with its concomitant effect, increased irritability, some direct cause of vascular distention appears requisite.

Now the means here employed appear well calculated to produce this effect. Removing the brain not only renders the nervous system less susceptible to the influence of the poison, and thus diminishes the tendency to capillary constriction arising from the irritation it occasions ; but decapitation, by altering the course of the blood, probably causes it to be sent with additional force to the spinal marrow, where it may accumulate in some degree and increase muscular irritability ; while, at

the same time, its general impulse may be augmented to an indefinite degree by artificial respiration, which mechanically forces it on to the heart, and excites that organ to increased action.

Thus it appears to be proved, both that the poison operates through cerebral influence, and by producing a change in the condition of the capillary vessels ;—first, because removal of the brain prevented its effects, by suspending the power of constriction in the vessels ; and, secondly, because the very reverse of those effects took place when means were employed calculated to produce an opposite state of these vessels, or that of capillary distention.

We shall proceed to consider next the experiments of M. Le Gallois, made for the purpose of ascertaining the source from which the heart derives its power of action.

The general inference which M. Le Gallois draws from them has been successfully combated by Dr. Wilson Phillip ; but these experiments present several curious and in-

teresting facts, and the explanation of these offered by M. Le Gallois involves other principles, the truth of which appears no less questionable than that of his general inference.

The consideration of these is the principal object for which the experiments are here adverted to ; and for this purpose it will not be necessary to give a detailed account of the whole ; a general view of them will be sufficient, with a more particular statement of such points only as relate to the object proposed.

M. Le Gallois has shown, that partially destroying the spinal marrow, instead of paralysing all the parts below the seat of the injury, as it was formerly supposed to do, directly affects those only which derive their nerves immediately from the portion of spinal marrow destroyed. Thus, for instance, destroying the cervical portion paralyses the neck and superior extremities ; destruction of the dorsal portion paralyses the trunk ; while the lower extremities are affected by destroying the lumbar portion.

The indirect participation of other parts in these local injuries is one of those points

which is not satisfactorily explained, and does not appear to have been duly appreciated by M. Le Gallois ; but still the science is indebted to him for a valuable addition to our knowledge in these important physiological facts ; from which some useful practical inferences may be drawn, relative to the application of remedies in certain local affections.

M. Le Gallois not only found that the sensibility and power of motion were lost in the parts affected, but also that circulation was suspended in them ; insomuch that amputating the thigh of a rabbit after destruction of the lumbar portion of the spinal marrow, caused not a drop of blood to flow.

Besides this, it also appeared that the action of the heart was enfeebled and retarded by this operation ; and that its action was impaired in proportion to the quantity of spinal marrow destroyed, with some slight variation arising from the age of the animal, and the particular part destroyed ; the heart being more immediately affected by the cervical, less so by the dorsal, and least by the lumbar portion being removed.

The general inference which M. Le Gallois draws from these facts is, that the heart derives its power from, and depends upon the whole of the spinal marrow; and accordingly experiences a diminution of power proportioned to the extent of the injury inflicted.

Finding the general circulation affected by partial injury of the spinal marrow, he sets out with the assumption, that no cause can be assigned for this but either a diminution or loss of power in the heart.

“ Si la destruction de la moelle epiniere arrête cette fonction, ce ne peut être que de deux manières, en faisant cesser les mouvemens du cœur, ou bien en les affoiblissant.”
P. 67.

It appears, however, in the sequel, that M. Le Gallois, in explaining the effects of the partial loss of circulation, compares the resistance opposed to the transmission of the blood through these parts, to that of an imperfect ligature round the vessels.

“ La destruction d’une portion de moelle insuffisante pour arrêter la circulation générale, la diminue toujours beaucoup dans

les parties correspondantes à la moelle détruite, et y fait jusqu'à un certain point l'office d'une ligature." P. 145.

Now it is not easy to imagine any state of vessels which can operate as a ligature except that of constriction or collapse; which, or something equivalent to which, is here admitted by M. Le Gallois.

And it is precisely on this principle, or constriction of the capillary vessels impeding the transmission of the blood and retarding its return to the heart, that the participation of this organ, and the leading facts in the experiments in question, appear to admit of explanation; without the necessity for supposing that the power of the heart is directly affected by the injuries of the spinal marrow. Or the action of that organ, in M. Le Gallois' experiments, might fail for want of stimulus to excite it to action, rather than from want of power to act.

Without maintaining that capillary action is indispensably necessary to support circulation, it may safely be asserted, that these vessels are capable of opposing more or less resistance to the blood passing through them,

according to their different states of relaxation or contraction. And sufficient proofs have been given to show that their degree of contraction is liable to constant variation, both from corporeal and mental impressions.

On this principle, then, it is presumed that an explanation may be offered, which is not only more consonant with the views established in the preceding inquiry, but which also accords better with the facts presented in these experiments. But capillary constriction being here produced in an unusual manner, and under circumstances of a peculiar nature, it is first necessary to call the attention to these points, in order to prevent misapprehension.

It has been formerly shown, that constriction of these vessels is the ordinary effect that results from fear and pain; and destroying a portion of the spinal marrow is an operation calculated to produce both. In fact, M. Le Gallois thus strongly expresses himself on this point: “L’instrument tranchant porté dans le siege même de la puissance nerveuse y cause une commotion dont ils (les animaux) ont souvent beaucoup de peine à se remettre,

et qui affoibloit toutes les fonctions. Il en resulte, qu'ils sont en général dans un état d'atonie assez prononcée."

There is then abundant reason for concluding, that the specific effect of fear and pain must attend the operation in question.

But it may perhaps be expected, that this effect, which is capillary constriction, should cease as soon as the spinal marrow is destroyed, if it be produced through nervous influence. This, however, is not a necessary consequence. Admitting the nerves to be essential to its production, cutting off their connexion with the sensorium does indeed instantly suspend the influence of the mind over the parts thus detached ; but it does not instantly deprive those parts of organic irritability, which is retained for some time after. And this constriction being the state into which they are thrown at the time of separation, it is reasonable to conclude, that it will continue until the period of contraction spontaneously subsides, unless some farther cause arise to change it.

Its continuance, however, will not, in all probability, be long, as any unusual con-

traction is soon followed by relaxation, even when the organs are entire. Still less, then, should we expect it to be durable in a state of mutilation. In short, it can only be regarded as the last transient effort of cerebral influence.

Again, it may be supposed that the sympathetic participation of the rest of the capillary system with this local change of action, should be prevented when the nervous connexion between the part primarily affected and the sensorium is intercepted by division of the spinal marrow.

This supposition involves the assumption which remains to be proved, that the nerves are the only direct means of propagating vascular sympathy; whereas it is possible that the vessels themselves may be adequate to that purpose.

But were the assumption admitted, and allowing the sensorium to be the only medium through which local change of action is communicated to all parts, still in this instance the objection would not be valid; for the sympathetic participation is not here supposed to arise after the destruction of the

spinal marrow is completed, but in the very act of inflicting the injury, from the alarm and suffering which the animal endures under the operation. And at this time the connexion is not yet intercepted, but cerebral influence still extends to the organs affected; although it may cease from the moment the separation is complete, and the consequences of the injury alone may then remain.

Having premised these observations, we may now proceed to inquire how the facts accord with the explanation proposed.

In the first place, M. Le Gallois found that destroying any one of the three portions of the spinal marrow in rabbits of twenty days old and upwards, if not effected with certain precautions hereafter to be noticed, was capable of checking the general circulation.

Now upon M. Le Gallois' view, that the heart derives nervous influence from all parts of the spinal marrow, it does not very clearly appear why its action should be wholly suspended by withdrawing only a portion of that influence.

But if this organ be affected by the constriction of the capillary vessels, impeding

the transmission of the blood, and preventing its return to the heart, and thus depriving it of its natural stimulus, the reason may be easily assigned.

From the general sympathy or consent of action that prevails throughout this system, any considerable local impression is liable to affect all parts ; and thus not only those organs which derive their nerves directly from the portion of spinal marrow injured, but others also will participate in the capillary constriction that arises from the fear and pain which the animal suffers under this operation. And thus the heart may be partially deprived both of its natural stimulus to action, and of the means of supporting circulation, by this obstacle to the transmission of the blood.

M. Le Gallois, however, found the circulation to be sooner suspended by destruction of the cervical or dorsal portion than by that of the lumbar.

The cause of this again, upon his view, is not very evident. If the heart derive its power from all parts of the spinal marrow alike, why should destruction of one portion

affect it sooner and more powerfully than that of another?

But if vascular constriction, impeding the return of the blood, be the cause, the reason may be easily explained.

The parts immediately connected with the seat of the injury will, of course, feel its effects sooner and more powerfully than those which participate indirectly or through sympathy. Now the heart receives its arterial blood from the lungs, the supply of which depends upon the pulmonary vessels, and these will participate in the impression on the nerves sooner when the cervical portion is primarily injured, than when the dorsal or lumbar is the immediate seat of the injury, and the cervical suffers secondarily.

M. Le Gallois did not find the effect to be instantaneously felt by the heart in any case, but only after an interval of some minutes.

Now the effect of injuring their nerves is instantaneously felt by other organs directly dependant on those nerves; then why should it not be by the heart also, if it be in the same manner dependant upon the spinal nerves?

But if the heart participate only indirectly,

from retardation of the returning blood, the effect will not be immediate in either case.

The immediate effect of general constriction of the capillary system will most probably be to throw an increased quantity of blood upon the lungs, and until the function of this organ fail, the heart will not be deprived of arterial blood, nor will circulation cease.

But what, it may be asked, will be the consequence, if the lungs be primarily affected? And we are indebted to M. Le Gallois for elucidating this point. The effect, as already stated, is much more immediately felt in this case; and the manner of it appears to be as follows.

The lungs appear to be most particularly affected by injury of the par vagum, which sends branches directly to that organ; and the following is the state in which they were found after the division of these nerves.

“Après la mort, on trouve les poumons boursoufflés, en partie gorgés de sang, en partie remplis d’un fluide sereux, et souvent ecumeux, et leur inspection montre clairement que l’air extérieur ne pouvoit plus y pénétrer, ou en très petite quantité.” P. 234.

These effects perfectly accord with the view here given of the vessels becoming constricted, and are not indeed easily to be accounted for otherwise.

It is not to be supposed that vascular constriction will equally affect all classes of the pulmonic vessels ; it is especially in the extreme branches, or in the capillaries, that it is likely to occur soonest ; and this, by impeding the transmission of the blood, accounts for the partial congestions in those of a larger class, which are found to take place, and noticed by M. Le Gallois.

Moreover, on secreting and exhaling surfaces another effect has been shown to attend increased contraction of the secretory and exhalent vessels, which is a relaxation of their mouths or ducts, and a consequent increase of thin watery secretion or exhalation ; hence the cold sweat that attends collapse of the vessels on the surface from fear, hæmorrhage, and syncope ; hence the copious secretion of watery fluid from the stomach attending nausea ; and in like manner an effusion of a serous fluid, or of thin frothy mucus, might be expected in the lungs, from the partial

collapse or constriction of its vessels ; and accordingly this also takes place.

The mechanical action of the lungs sufficiently accounts for this fluid becoming frothy ; and its gradual accumulation, as M. Le Gallois justly remarks, clogging up the bronchiæ and impeding respiration, appears to be one of the immediate causes of death. He has shown, that division of the par vagum is liable to operate also in another way, namely, by narrowing the aperture of the glottis. But this does not preclude its action on the pulmonary vessels, which M. Le Gallois admits, and very aptly compares to the effect of injuring the nerves of other organs, producing a sort of paralysis of these vessels. There is, however, an ambiguity in the use of the term paralysis, which it is necessary to guard against. Paralysis is rather the relaxation from loss of power, consequent to the destruction of the nerves ; whereas, the state of vessels here spoken of is the last effort of vascular constriction that precedes this state of relaxation.

To proceed, M. Le Gallois found a remarkable difference resulting from the man-

ner of destroying the spinal marrow. When a large portion was destroyed at once, the circulation was more suddenly arrested; but when smaller portions were destroyed successively and at intervals, the destruction might be carried to a much greater extent without producing that effect.

Here again, if loss of power in the heart from abstraction of nervous influence be the cause that enfeebles circulation, we should expect the loss of power to be proportioned to the quantity of nervous influence withdrawn; and are at a loss, on this principle, to explain the difference from the manner of performing the operation. But if the general impression on, or the shock given to the nervous system, affecting the capillary circulation be the cause, we may easily account for it.

Any sudden or violent impression, as shown in the effect of narcotic poisons, may produce a general and instantaneous constriction of these vessels; so that the heart and lungs may be overpowered by the blood suddenly thrown upon them, and instant death may be the consequence.

But the repeated application of less powerful impressions may be borne without this effect; if each singly be inadequate to produce it, and if sufficient time be interposed for the effect of the first shock to subside before the second is inflicted, and so on. And thus the difference in question may be accounted for.

M. Le Gallois' mode of explaining it will be considered presently.

M. Le Gallois further found, that partial destruction of the spinal marrow no longer suspended the general circulation, if a ligature were previously thrown round the larger vessels, so as to prevent the blood from circulating through the parts connected with the injured portion of the spine. And conceiving that the force required to keep up circulation in the remaining parts was lessened in the same proportion that its general extent was diminished, he regarded the ability of the heart to perform its task after the ligature was applied, as a conclusive proof that loss of power prevented it from doing so before, and that the ligature acted by facilitating circulation.

Now, whether thus narrowing the channel through which the blood has to pass would lessen the force required to propel it, may perhaps admit of a doubt; but if the object were to prevent retardation, and not to lessen the propelling force required, the ligature appears well adapted to this purpose, or to accelerate the returning blood. For preventing admission of the blood into parts where its transmission would be retarded must force it to seek other channels, where no such obstacle occurred to impede its progress.

This mechanical explanation appears, however, still less satisfactory than another which is suggested by an important fact stated by M. Le Gallois himself.

One effect, which was evidently found to result from a ligature round the aorta immediately below the coeliac artery was, as he states, that it lessened or destroyed the sensibility of the lumbar portion of the spine, before the injury was inflicted on it. So that the rabbit, as M. Le Gallois says, which testified great pain when the instrument touched the dorsal portion of the spinal marrow, no longer showed any signs of suffering when it

reached the lumbar. The cause of this insensibility in the lumbar portion M. Le Gallois with reason ascribes to its loss of circulation produced by the ligature; the sensibility of the spinal marrow depending, as well as that of every other organ, on its state of circulation.

Now if the spastic constriction of vessels depends upon the alarm and suffering produced by the injury inflicted, we may readily perceive how the latter at least is lessened by previously destroying the sensibility of the part injured.

The explanation M. Le Gallois offers of the effect of the ligature, has already been stated, namely, by its facilitating circulation; and on the same principle he accounts for the animal being better able to bear the successive destruction of different portions of the spinal marrow, than the sudden destruction of a large portion at once. That is, he conceives each portion to act the part of an imperfect ligature, partially precluding the admission of the blood into this part, and thereby limiting the extent of the general circulation.

To this it may be replied, that M. Le Gallois is here changing his ground at least, if not giving up his own argument, that the circulation fails from “want of power in the heart,” and not from resistance in the vessels. But if this vascular constriction, acting like a ligature, facilitate circulation by its operation on one part, why not also on another? Or should not the facility thus afforded be greater the more extensive its operation? But the inadequacy of this explanation will further appear hereafter.

M. Le Gallois found, that by previously decapitating the animal, and artificially inflating the lungs, he could destroy any portion of the spinal marrow without interrupting the circulation.

This M. Le Gallois explains in the same manner as the ligature, by its limiting the extent of circulation. But it appeared, in former experiments, that when the cervical portion of the spine was destroyed, the blood was frequently not sent with force enough to fill even the carotids; consequently it could not be the extent of circulation that op-

pressed the heart, when it failed in the very outset.

The explanation already offered appears more satisfactory. Injuring the cervical spine cuts off the supply of arterial blood, and thus stops circulation. But the brain being the primary organ of animal life, its removal prevents completely the alarm and sensible pain which the animal suffers under the operation, and thus suspends their influence. Moreover the brain is the centre of the nervous system, and commonly considered as the medium through which local impressions act generally. Thus its removal is calculated to prevent that sympathy to which the effects have been chiefly ascribed. Can we then wonder that its removal should so materially modify the result of these experiments?

But having noticed the principal facts related in M. Le Gallois' experiments, we may proceed to consider those of Dr. Wilson Philip, in order to show that they also tend to confirm rather than refute the views which have been offered.

Dr. W. Phillip repeated several of M. Le Gallois' experiments, and confirms their accuracy; yet from others nearly similar made by himself, he obtained results so different as to lead him to draw from them inferences the very reverse of those drawn by M. Le Gallois.

M. Le Gallois had found, after destroying the lumbar portion of the spinal marrow, in rabbits twenty days old and upwards, that the circulation was suspended, so that amputating the thigh caused not a drop of blood to flow.

On the other hand, Dr. W. Phillip found, after total destruction of the spinal marrow, that circulation continued, and the blood flowed as freely as before.

M. Le Gallois found, in the same experiment, that inflating the lungs within three minutes and a half after destroying this portion of spinal marrow, had no effect in restoring circulation, though continued for ten minutes.

On the contrary, Dr. W. Phillip kept up circulation for eleven minutes by inflating the lungs of a rabbit after total destruction

of the spinal marrow ; and circulation at length appeared to cease, only because the whole of the blood was nearly expended from different arteries that had been opened.

Similar difference appeared in the result of other experiments related by Dr. W. Philip ; nor has he satisfactorily shown the reason of this difference, although he expresses no doubt of the accuracy of those performed by M. Le Gallois.

He seems to ascribe this difference chiefly to respiration being impeded in the experiments of M. Le Gallois, which, he says, is a complicated function, and fails as quickly if any one of the powers essential to it be withdrawn, as if they had all ceased. But we find that M. Le Gallois constantly resorted to artificial respiration, and that, as stated above, without effect.

When we look to the mode of performing the experiments in question, we are struck with a point of difference, although it is one which Dr. W. Phillip assures us does not at all influence their result (P. 70) ; and this is, previously destroying the sensibility of the brain, by a blow on the occiput.

Now this is a circumstance which, from the preceding view, we should expect to modify the results so materially, that, until Dr. W. Phillip offers some convincing proof of the contrary, we must conclude that it does affect their result, and on this point be allowed to question the accuracy of his opinion.

In the experiments of Mr. Brodie the effect of certain narcotic poisons was found to be totally reversed by cutting off the head and employing artificial respiration ; and M. Le Gallois, though he assigns a different cause for this effect, yet he also states, that “ the surest way to prevent circulation from being suspended by destruction of the lumbar portion of the spinal marrow, is by previous decapitation.”

Now although destroying the animal sensibility by a blow on the occiput may not be in all respects equivalent to total removal of the brain, yet there is every reason to believe that its operation will be similar in kind, though perhaps minor in degree.

Something will no doubt depend upon the degree of injury inflicted on this organ, whether its sensibility be totally destroyed, or only

partially ; for this latter appears to have been the case in some of Dr. W. Phillip's experiments, in which its organic irritability remained, as it was still susceptible to the influence of certain stimuli. In other cases this organ was totally crushed by the blow of a hammer, and then the effect was such as M. Le Gallois found to result from suddenly destroying a large portion of the spinal marrow, or circulation was instantly suspended by the shock.

The first effect we should look for from destroying the animal sensibility by a blow on the occiput is, that the same capillary constriction would ensue from it as from crushing the brain, though in a less powerful degree. And such appears to have been the case.

Accordingly Dr. W. Phillip states, (P. 75,) that the circulation often failed before either the spinal marrow or the brain was removed.

But the immediate effect having subsided, and circulation being no longer impeded by any resistance excited through cerebral influence, while respiration was supported by artificially inflating the lungs, it may easily

be conceived, upon the view already offered, cerebral influence being withdrawn, that the same consequence would no longer result from any subsequent impressions upon the nervous system.

Accordingly, Dr. W. Phillip now found that he could remove the whole of the spinal marrow, and also the brain, without circulation being suspended.

Although the apparent contradictions may thus be reconciled, still these facts are sufficient to invalidate the inference of M. Le Gallois, as they certainly show that the heart is much less dependant upon the spinal marrow than he supposed; its action continuing for a certain time at least after total removal of that organ.

But whether the inference Dr. W. Phillip draws be fully borne out, namely, that the heart and all other involuntary organs, (for the same power was found in all,) are wholly independant of all nervous influence whatever, and that their power of action resides entirely in themselves, may still admit of a doubt.

It may be questioned on the ground that

the nerves entering into the structure, and forming part of these organs, retain their power for a certain time after their separation from the source of nervous influence, as admitted by Dr. W. Phillip, P. 226.

“ Although we have reason to believe, from every observation on the subject, that the brain and spinal marrow are the only sources of nervous influence, yet it is evident that a certain portion of this influence remains in the nerves when separated from those organs.”

And the inference of Dr. W. Phillip may also be questioned on the ground, that the influence of the ganglia remained, although that of the brain and spinal marrow was withdrawn.

But while Dr. W. Phillip denies the dependance of involuntary motion upon nervous influence, he does not dispute the liability of internal organs to be occasionally affected by external impressions ; he even conceives this to take place by a direct conveyance of the impression from the part that receives it, through the medium of nerves, to the part affected.

This direct conveyance of external impressions to the heart and abdominal viscera is the point we are most interested in disproving. And here it may be remarked, that questioning their liability to be directly affected by impressions on the nerves is not incompatible with the admission that these organs ultimately derive their susceptibility of impression at least, if not their contractility, from the nerves entering into their structure. On the contrary, these two points have generally been taken in conjunction by those who regard it as one of the principal offices of the ganglia to ensure a regular and uniform supply of nervous energy to involuntary organs, the regularity of whose function is so essential to life; and at the same time to preclude them from experiencing those sudden shocks to which they would be exposed were they directly sensible to external impressions on the nervous system.

Whether these organs be liable, however, to this direct impression, or whether the effects related be more satisfactorily explained upon the view of previous changes in the ca-

pillary system, let the facts before us determine.

In the first place, then, Dr. W. Phillip found in his experiments that the heart was not influenced by impressions on the brain or spinal marrow, unless they acted upon a considerable portion of these organs. Thus it appeared to be rather the extent than the intensity of the impression that was material to this effect.

Now if the heart feel the impression directly, there seems no reason why this should be the case. But if this organ be indirectly affected through previous change of capillary circulation, we may readily conceive why, after the sensibility of the brain was suspended, and its organic irritability alone remained, the impression could no longer produce change of action in those vessels unless extensively applied to them, as when acting on a large surface.

On the same principle may be explained also the following fact. Dr. W. Phillip found more effect from applying spirit of wine than from any mechanical injury (P. 107); and in

general, chemical were far more powerful in their influence than mechanical impressions.

Now if the extent of surface acted upon produce the effect, this is easily accounted for; but if it proceed from lesion of the nervous structure, mechanical injury should be more felt than the mere application of spirit of wine, which causes no breach of texture.

In the next place, Dr. W. Phillip found it perfectly indifferent to what part of the brain the impression was applied. This one could hardly expect to be the case were its influence conveyed directly to the heart;—nor was this the case with those organs which are allowed to be directly subject to nervous impressions, as the organs of voluntary motion. But if the effect was communicated through the intermediate influence of the capillary vessels, it would be immaterial to what part of the cerebral substance the impression was applied, as the capillaries pervade all parts.

Moreover Dr. W. Phillip found the effect to be just the same when applied in the same manner to the spinal marrow as to the brain;

and so we should expect it to be on the preceding view, but not otherwise.

Further, Dr. W. Phillip found that he could not by any mode of irritation applied to the sensorium render the action of the heart irregular, but could only accelerate or retard it. Now if the impression be directly felt by that organ, it might surely be so applied as to act irregularly. But if it act through the medium of the previous changes in the capillary system, these changes would necessarily be restricted to two modes of operation, that is, to accelerating or retarding the returning blood, as the vessels become relaxed or constricted; and the effect on the heart would correspond.

Again, the immediate effect of the impression being either constriction of vessels or their relaxation, according to the substance applied and its mode of application, if the indirect influence this exerts upon the heart, in accelerating or retarding its action, be ascribed to nervous influence, then must the nervous influence be considered stimulant at one time and sedative at another.

Accordingly, Dr. W. Phillip, (P. 243), comes to this conclusion, "that the nervous influence may be either stimulant or sedative to the heart and vessels." And he moreover states, "that the sedative effect is not the result of excess of stimulus, but the direct operation of the agent," P. 244; which surely involves a contradiction.

But, in short, we find that Dr. W. Phillip performed one experiment, the result of which appears perfectly conclusive against direct nervous communication.

He removed the whole of the spinal marrow in rabbits, and in frogs the base of the brain also, so that all direct nervous communication between the sensorium and the heart was effectually intercepted. He then applied the stimuli to the remaining part of the brain between the eyes; and he still found the effect upon the heart exactly the same as when the brain and spinal marrow were entire. (Exp. 18. p. 84, and Exp. 23. p. 90.)

Surely these facts afford sufficient reason, to conclude, that the impression was not communicated directly but indirectly to the heart; and if they do not prove the capillary vessels

to be the intermediate organs through which the impression affected the circulation and altered the action of the heart, by facilitating or impeding the returning blood, they must be admitted strongly to favour that conclusion; as the only medium of communication left in this case was mere continuity of substance.

While the heart thus appears to be exempt from direct participation in impressions on the nerves, there is reason to conclude that the other involuntary organs are equally so. The principal fact, from which the opposite conclusion has been drawn, is the occasional derangement of function which the organs of secretion and digestion experience under the influence of the passions. But this circumstance admits of more satisfactory explanation from their manifest connexion with, and dependance upon, the state of the capillary circulation.

Dr. W. Phillip has indeed rendered it probable that the nervous influence contributes, by chemical decomposition of the fluids, to the processes of secretion, digestion, and the evolution of animal heat; but he does not

the less admit the dependance of these functions upon the state of capillary action. And so far his views do not militate against those offered in the preceding pages.

There is one point indeed in which his reasoning is at variance with the views that have there been given. He seems to consider the continuance of circulation through the capillary vessels as a sufficient proof of their action continuing, and its cessation as a proof that capillary action has ceased. But neither of these is a necessary consequence ; for the blood may flow through these vessels as through mere passive tubes after their power of active resistance has been destroyed, as appears to have been the case in most of Dr. W. Phillip's experiments ; and on the other hand, the transmission of the blood through these vessels may be prevented by the very exertion of that power, or by the resistance they oppose, in a state of constriction ; as appears to have been the case in most of M. Le Gallois' experiments. And from overlooking these points arises the apparent contradiction between the experiments

of these two gentlemen ; Dr. W. Phillip reasoning altogether from the ultimate effect of withdrawing the nervous influence, which appears to be relaxation of vessels ; while the reasonings of M. Le Gallois were all founded upon the immediate effect of the injury inflicted, which appears to be vascular constriction.

Nor without attention to this difference can several of the experiments of Dr. W. Phillip be reconciled with each other, their results appearing contradictory ; or one effect ensuing when the brain was allowed to retain its influence, and the opposite effect when that influence was withdrawn.

Thus, for instance, on the subject of animal temperature, this apparent contradiction repeatedly occurs. Exp. 60, p. 175, leads Dr. W. Phillip to the conclusion, that the destruction of any considerable part of the spinal marrow lessens the temperature of the animal.

Whereas, on the other hand, it appears from Exp. 81, 82, 83, p. 224, that the rate of cooling, or the loss of temperature, when

the animal is dying, is not the least altered, whether the brain and spinal marrow be totally destroyed or left entire.

The experiments are as follow. See P. 175, Exp. 60.

The lumbar portion of the spinal marrow was destroyed in a rabbit two months old, and the temperature measured by a thermometer held in the mouth for two minutes at a time.

Immediately after the operation . . . 98°

In twelve minutes after it 92

In half an hour after it 92

In two hours and a half 98

In five hours and three quarters . . . 98

In seven hours and a quarter . . . 98

In nine hours 96

In ten hours 95

The animal during all this time appeared lively, and ate parsley.

In eleven hours 96°

In twelve hours 97

Night coming on, the temperature was not measured again for thirteen hours. In the morning the rabbit appeared lively and ate readily.

In twenty-five hours . . . thermo. 88°

In twenty-seven hours 84

In twenty-nine hours 88

In thirty hours 84

In thirty-one hours 84

In thirty-three hours 80

The animal still continued to eat.

In thirty-four hours 75

In an hour after the animal died."

From this experiment Dr. W. Phillip concludes, that partial destruction of the spinal marrow lessens the temperature. P. 249, 35.

On the other hand, the following results were obtained from Exp. 81, 82, 83, p. 224.

"Exp. 81.—Two rabbits of the same size, whose temperature was 98°, were killed in the usual way (by a blow on the occiput.) In one, immediately after death, the brain and spinal marrow were destroyed by introducing through a hole in the cranium a wire of nearly the same diameter with the cavity of the spine, repeatedly pushing it on to the end of this cavity, and then moving it about for some time in the cavity of the cranium. The other

rabbit was left entire. A hole was made about the centre of the abdominal muscles in each to admit of a thermometer being introduced into the cavity of the abdomen. They were placed near each other in a temperature of 50° . During the first twenty minutes each lost exactly 4° , and they both lost during the succeeding three quarters of an hour just 2° during each quarter. Something, which we could not ascertain, accelerated the rate of cooling during the next quarter, and so exactly did it correspond in both rabbits, that each lost during this quarter 2.5° . After this their temperature diminished more slowly, and still more so of course as it approached more nearly to that of the air, but still in both it was found to correspond. At the end of a hundred and ten minutes the temperature of both rabbits was 84° ."

"Exp. 82.—The foregoing experiment was repeated, with this difference, that in both rabbits the lungs were inflated; but we could not perceive that one rabbit cooled faster than the other."

The results of these two experiments cer-

tainly appear to be at variance with those of Exp. 60. But if considered in the light that has been stated, the cause of this difference may be accounted for.

In Exp. 60, the brain retained its influence, and the ordinary effect of the injury inflicted, or capillary constriction, ensued ; and this diminishing circulation reduced the temperature for a time.

How long this effect continued Dr. W. Phillip does not exactly say, but it did not exceed two hours and a half ; a very material point, not explained by Dr. W. Phillip, but readily accounted for on the principle here proposed. The power of constriction being exhausted, relaxation succeeds, and circulation returning, the temperature is restored. It is deserving of notice, that the cold stage of fever, when violent, seldom continues longer than two hours. It is probable that some febrile reaction in this case accompanied the return of free circulation, and helped to restore the temperature. But the powers of life being enfeebled, the action of the heart began to fail, the temperature soon declined again,

and with partial fluctuation, easily to be accounted for, continued to fall till the animal died.

If we now revert to Exp. 81 and 82, we find, that in these the influence of the brain was withdrawn, the animal sensibility being previously destroyed by a blow on the occiput; thus the constriction was prevented from taking place, and the same fluctuations of temperature therefore no longer followed.

These experiments are moreover important, as they show, that destroying the sensibility of the brain is nearly equivalent to its removal by decapitation, in preventing the immediate effects of destruction of the spinal marrow; for that destruction appeared afterwards to have no longer the same influence as before in producing capillary constriction.

The results are equally contradictory with regard to the effect of inflating the lungs, which in the one case was found to occasion no difference in the rate of cooling, whether the brain and spinal marrow were removed or retained; whereas in other cases, cerebral influence being retained, a considerable fluctuation of temperature resulted from artificial

respiration, which accelerated or retarded the cooling of the animal according to the manner in which it was performed.

When the air thrown into the lungs was in a certain quantity only, the temperature was supported by it; but, on the contrary, the heat was reduced, instead of supported, by throwing more air into the lungs than was required for arterializing the blood that passes through them. From observing this, Dr. W. Phillip was led to a conclusion which had been previously drawn by Dr. Crawford, who states, in conformity with his theory of animal heat, that respiration may be directly either a cooling or a heating process, according to circumstances. (Crawford on Animal Heat, p. 386.)

Were it not foreign to our purpose at present, arguments might be adduced from Dr. W. Phillip's experiments against his inference, that involuntary motion is independent upon nervous influence; especially from the fact, that internal organs are liable to become relaxed and greatly distended, if the animal be allowed to live any considerable time after the nervous influence is withdrawn from them.

Thus the vessels of the lungs are commonly found in a state of congestion, and the stomach is generally, as Dr. W. Phillip informs us, found greatly distended after the eighth pair of nerves have been divided. The rectum was also found greatly distended from destroying the lumbar portion of the spinal marrow; and the bladder in one case was actually burst from over-distention.

These are certainly effects that denote the loss of contractile power, and cannot be explained, on Dr. W. Phillip's principle, by the want of nervous influence preventing the proper chemical changes in the secreted fluids from taking place.

Secretion was not suppressed, or the bladder could not have been ruptured; it might be vitiated, as it naturally would be, by withdrawing the nervous influence, since the action of the vessels will thus be altered; but it will only be suppressed when these vessels are constricted. Their subsequent relaxation allowing the passive transmission of the fluids will only alter its quality, but not diminish its quantity. On the contrary, its quantity may be increased by this relaxation, and actually

appears to be so in various affections attended with loss of power and action in the vessels ; as, for instance, in colliquative sweat, in colliquative diarrhœa, in cholera morbus, in certain hysterical affections, and in diabetes. Thus it is far from a necessary consequence, that circulation and secretion must cease from the loss of capillary action ; or that circulation through these vessels continuing, proves them to retain their power of action.

In endeavouring to show the uncertainty of conclusions drawn from experiments on living animals, and the consequent unfitness of such conclusions to form the basis of physiological science, it is by no means the writer's wish to depreciate the utility of such experiments generally, nor the particular merit of those under consideration. His object is merely to answer such objections to his own views as might be drawn from them ; and at the same time to vindicate the mode of research which he has pursued, by showing that experiments on living animals, which are attended with pain, fear, and mutilation, instead of presenting facts less fallacious, or more instructive, than the ordinary pheno-

mena of nature, more frequently perplex than simplify the questions they were meant to elucidate.

In every science, the more simple problems are the first to be solved, but the results thus obtained are generally the most complicated that the science presents, and should therefore be the last, instead of the first, that the physiologist undertakes to explain. Absolute reliance, indeed, can never be placed on any inferences drawn from them, until the science has attained to the utmost degree of certainty and perfection, for till then it cannot be known what sources of fallacy may still remain undetected.

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